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Psychological Monographs.

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THE

Psychological Review

EDITED BY

HOWARD C. WARREN
PRINCETON UNIVERSITY

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(*Editor of the Psychological Monographs*)

Studies from the Psychological Laboratory of the University of Illinois

Vol. I., No. 1.

EDITED BY STEPHEN S. COLVIN

THE REVIEW PUBLISHING COMPANY
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INTRODUCTORY NOTE.

The following pages present the completed experimental investigations of the Department of Psychology of the University of Illinois carried on during the past few years. The work now in progress includes investigations in the psychology of sensation, in animal psychology and in the psychology of the learning processes. This latter series of investigations is being conducted both in the psychological laboratory and in several of the public school systems of the State. It is the purpose of the department to emphasize more and more this phase of its work. Because of an unavoidable delay two extensive studies by Professor J. W. Baird of the department were prevented from appearing in the present monograph. They will be published in a second monograph from the department and this it is hoped will be issued during the coming year.

S. S. C.

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THE COLOR PERCEPTION OF THREE DOGS, A CAT AND A SQUIRREL.

BY STEPHEN S. COLVIN AND C. C. BURFORD.

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I. REVIEW OF THE LITERATURE ON THE COLOR PREFERENCES OF ANIMALS.

The general question of the preference of animals for colorless and colored light has been the subject of speculation and investigation for the last forty years. It was maintained by Darwin and others[1] that the color of flowers attracted insects, especially bees, and was a great aid in cross pollination. Wallace also speaks of color preferences of certain animals. Grant Allen (2) in an extended treatise on the color sense speculates about the color reactions and preferences of animals and insects, but his treatise is not on a scientific basis.

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[1] H. Müller, 'Alpfenblumen, ihre Befructung durch Insekten und ihre Anpassungen an dieselben.' Leipzig, 1881. For a later discussion see Andreae, E., 'Inwiefern werden Insekten durch Farbe u. Duft der Blumen angezogen?' *Beihefte z. botan. Zentralb.*, XV. (1903).

The first experimental work performed in this field was by Paul Bert (6) in 1869. He worked with *Daphnia* and tested their light and color reactions by means of the solar spectrum. He found that these little animals congregated principally in the yellow-green region. He concludes that all the animals tested saw only the same spectral rays as we see; he also asserted that through the whole range of vision brightness differences of the same colored rays are identical for them and for us. He attributed the preference for the yellow-green end of the spectrum to a liking for the greater brightness and not to any actual color preference. But he made no attempt to isolate and separate the brightness and the color factors.

A more thorough piece of investigation was performed by Sir John Lubbock (33). He investigated the color and light reactions of *Daphnia pulex* by a method similar to that which was employed by Bert, though he took much greater care. He found that the greatest preference of these animals was for green; he also tested their limit of light sensitivity and found that they reacted to the ultra-violet rays, which showed a pronounced effect as compared with black, although this effect fell off rapidly as the rays approached their outer limit. He also tested ants to find how they were affected by different colored lights and media. The ants showed an antipathy for the violet end of the spectrum and particularly for the ultra-violet. He found in one test that when all the pupæ had been removed from the ultra-violet zone in which they had been placed, the ants then directed their attention to the violet zone, some pupæ being carried into the dark and some into the red and yellow zones. He further found that the ants chose blue in preference to the ultra-violet, thus disproving Bert's statement that animals see only the spectral rays that are visible to us. Lubbock also experimented on bees and found certain color preferences. The chief defect in his method was that he did not carefully distinguish between intensity and color stimuli.

Merejhkowsky (35) tested lower crustaceans with a somewhat more careful method and concluded, because they did not react to color differences, that they were unable to distinguish color. He found that they exhibited a light preference.

He states that they distinguished well the intensity of the ether vibrations, their amplitude, but not their length; that there is a great difference between the inferior crustaceans and man in the manner of the perception of light, for while we see different colors they do not see a single color in its various intensities. We perceive color as color, they perceive it as light.

The most careful series of experiments in this particular field were performed by Vitus Graber (18, 19, 20) in the early eighties. His principal investigations are described in a monograph of some three hundred pages. In these he tested the light and color reactions of more than fifty different animals, including the pig, dog, cat, rabbit and porpoise among the higher vertebrates; the gold-finch, common sparrow, bullfinch, raven, dove, cockatoo and guinea-hen among birds; the triton and the frog among amphibians; the lizard, the slow-worm, and the terrapin among reptiles; a large number of insects including bees, ants, crickets, fleas, house-flies, also spiders and a number of worms. These animals were tested for the most part with two colors at a time and their preference was determined by the color which they sought. Also some experiments were made by presenting several colors at once. The colors used being pure red, yellow-green, and blue with ultra-violet and black and white.

Some of the specific questions which Graber attempted to investigate were the following: (1) To what extent do animals distinguish degrees of light intensity, and what degree is most agreeable? (2) How far do animals distinguish different colors and which are most agreeable? (3) Has the visible spectrum in the case of certain animals a greater extent than in the case of man, that is, are ultra-violet and likewise infra-red visible to them and do they appear to them as particular qualities? (4) Are certain animals blind to parts of the spectrum visible to us. He found that among the animals tested by him only a few failed to react to color, while the remainder showed a pronounced, in some instances an astonishing, preference for certain qualities of light. Most of the animals that failed to react were high up the scale. Among these were the cat, the porpoise, the rabbit, the hen, the parrot and the tortoise. He found among dogs only certain animals that showed a marked light or color reaction; others seemed entirely indifferent. He concludes that there is no general agreement in color preference among animals; that among some animals there is no absolute color preference, but that the preference varies according to the arrangement of the colors compared; that the contrast effect of two colors is greater in proportion to the difference of the wave-length of the colors; that most animals possess some sensitivity for the ultra-violet rays; that with a few exceptions animals with a preference for white also have a preference for blue; that color causes pleasurable emotions in animals or the opposite, and that we are justified in speaking of a color taste (*Farbengeschmack*).

Graber also conducted a series of experiments with certain sightless and blinded animals. The results here were pronounced, in some instances almost startling. His first experiment with the common earthworm showed that it was sensitive not only in the anterior portion as had been previously held, but over its entire body, and this even when it was in a mutilated condition. He also worked with the blinded triton and with the blinded cockroach.[2]

This investigator later carried on a series of experiments with a number of marine animals, confirming in general the results of his previous researches.

A large amount of attention has been directed to the study of the effect of light and color upon the reaction of organisms far down the biological scale. In 1879 Englemann (12, 13) published a study on the stimulation of contractile protoplasm by sudden illumination, and three years later another on the light and color perception of lower organisms. In 1878 Strasburger (54) discussed the influence of light and warmth on swarm-spores. He found positive phototaxis for all colored swarm-spores in weak light. All swarm-spores do not react in the same way. Some showed positive reactions in strongest lights, but most showed a negative reaction if light is sufficiently intense.

---

[2] He attributed this sensitivity in the case of eyeless and blinded animals not to a chemical activity, but to an actual dermal sensitivity to the light rays.

Fatigati (15), in a study of the influence of various colors on the development and respiration of infusoria, concludes that the respiration of these animals is more active in violet than in white light and less active in green than in white.

During the last ten or fifteen years the literature on the subject has multiplied until it is very extensive, and no attempt will be made in this study to cite more than characteristic investigations here and there, and to state some general results. It has been found that amœbæ show definite reactions to light, their movements being checked by a light of strong intensity, white light causing them to come to rest. Blue affects them in the same way. Red light, according to Englemann's experiments, does not interfere with their activity.

Harrington and Leaming (22) state that amœbæ stream in the presence of red light but streaming is retarded, stopped or reversed by rays from the violet end of the spectrum. The effectiveness of various lights as inhibitors of protoplasmic flow diminishes in the order named; white, violet, red. Enucleated amœbæ stream in red light and cease to stream in violet or white light.

Wilson (62) studied the heliotropism of *Hydra,* and found that these animals collect in blue light but show no other color preference, except a slight one for green. Blue is preferred to white. The animals are restless except under these conditions. The animals move toward the light side of a vessel in a direct way suggesting purpose. In powerful light *Hydra* show negative reaction. Hertel (23), after experimental investigation of the influence of the ultra-violet rays upon *Hydra* and *Paramecium,* found that *Hydra* contract under the influence of these rays, which soon kill them. *Paramecia* do not react to ordinary light but they are sensitive to the ultra-violet rays. He also found that colored infusoria show positive and negative reactions to light. Stentors react negatively and *Euglena* positively.

Jennings (30) states that most bacteria do not react to light but " the purple bacteria are sensitive in different degrees to lights of different colors, tending to gather in certain colors more than in others. The largest number collect in the ultrared rays. There is another collection in the yellow-orange, while a few are scattered through the green and blue. None are found in the red, the violet or ultra-violet. . . . It is a most interesting fact that the colors in which the bacteria collect are exactly those which are most absorbed by them, and are also those which are most favorable to their metabolic processes."

G. H. Parker and L. Arkin (40) conclude that the earthworm *Allolobophora fœtida* is negatively phototactic to light of moderate intensity. These results are also in accord with the findings of Miss Smith (53). Adams (1) concludes that earthworms are negatively phototrophic in strong lights but positively phototrophic in faint lights. He says: "Earthworms retreat into their burrows during the day time because of their negative phototropism. They emerge at night, not so much because of darkness as because of the positive phototropism of faint light."

Among investigations on the influence of light on sightless animals during the last few years may be mentioned that of Parker and Burnett (41). They conclude that "Planarians without eyes react to the directive influence of light in much the same way as those with eyes."

Among investigations since the work of Graber on insects is that of Nagel (38). He handled the results of Graber critically and doubts the legitimacy of the conclusion that the reaction of blinded and eyeless animals to light is a genuine dermal light sensitivity. He believes that the reactions may be explained as due to chemical excitation.

Investigations of the Peckhams (42) in regard to spiders include a test of their color sensitivity. They conclude that these animals see colors, and find an indication of a preference for red, but no positive dislike for blue.

In 1896 Axenfeld (4) tested the influence of the Röntgen rays on insects. He found that the fly was very sensitive to these rays and reacted to them negatively.

Yerkes (63) in a careful study of the reactions and reaction time of medusa *Gonionema murbachii* to photic stimuli concludes that increase in light intensity uniformly causes a motor reaction in quiescent individuals and the inhibition of movement in active individuals. Decrease in light intensity usually causes the inhibition of movement in active animals, but rarely does it act as a stimulus in the case of resting animals. Strong light is injurious to *Gonionema,* and a few hours' exposure is usually fatal.

Among recent studies of the response of higher animals to light may be mentioned that of Ellen Torelle (58). In her experiments with the frog, she concludes that these animals "respond to light coming from above and from below as well as from the side. When a bank of sand is interposed between the frog and the light they crawl over this and move to the source of light. The frogs turn away from red light and move towards blue light. They move toward green and toward yellow light, but are not definitely orientated by either. When a red light is admitted at one end and green light at the other end of a receptacle the frogs move from the red to or toward the green. When red and yellow lights are apposed in the same way, movement is from the red to the yellow. When red and blue are apposed, movement is immediately toward the blue. When white light is admitted at one end of a receptacle, and the frogs are given a choice of a red or a blue environment, they move, in most cases, into the blue, and remain in it longer than they do in the red. When one half of the entire receptacle is blue and the other half is red (no white light being admitted), movement is from the red to the blue."[3]

Rouse (49) in 1905 made a study of the respiration and emotion in pigeons. He tested the animals in regard to their color preference and likewise in regard to their reactions to light as shown by a detailed study of their respiration. The colors selected were red, yellow, green and blue. The results show no pronounced color preferences, nor reactions for the various animals tested. It may be mentioned in this connection that Graber's study with these animals showed negative results as to intensity and color preferences.

Kinneman (31) in 1902 published an experimental study on the mental life of monkeys. In this he made a test of their color preferences by feeding the monkeys colored candies after the method of Garner (16), but reached no definite results. Another test was made under more favorable circumstances and with a different method. One of the animals tested, a female, showed no indication of preference. The other, a male, "appears to have a preference for bright colors, but blue seems to be discriminated against."

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[3] Graber, in his experiments, found frogs to be negatively heliotropic, and to prefer red to blue.

Garner in summing up his tests states that "all monkeys do not select the same color at different times, but I think that bright green is a favorite color with the Capuchins, and their second choice is white." In experiments with the Cebus monkeys and a few Macaques he concludes that "they are generally attracted by all brilliant colors."

The color preferences of infants have been studied in connection with their color discrimination. Preyer believes that yellow comes first with brown and red later; green and blue last.

Binet[4] (8) places red first in the series. Compayré concludes that yellow and red are the two colors most easily recognized by little children and that the child likes soft colors less than strong ones.

Baldwin (5) in his test with his children gives the following order of preference: blue, red, white, green, brown. He made no test with yellow, and found but slight difference in the attractiveness of blue and red.

Nagel (39) more recently conducted a series of experiments in color discrimination with his infant son, then two years and four months old.

"The child at once designated red, not only the spectral red, but likewise bright and dark as well as unsaturated reds; he included in his choice also pink, purple and occasionally orange.

"Green was learned without much practice and was not afterward forgotten; similarly, a few days later, violet and black.

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[4] Binet's order of preference is red, blue, orange, rose, violet, green, white and yellow.

"The recognition of gray and blue developed rather more slowly.

"Blue was forgotten again in two days; that is the name was not used, although the color was only very infrequently confused with other colors." Nagel gives the following scale of preferences:

Green
Violet } About equal in rank in the series.
Red
Black.
Yellow
White } About equal in rank in the series.
Gray
Blue.

Recently considerable discussion has developed in regard to the interpretation of the reaction of various lower organisms to light and to other stimuli. Davenport (11) in his Experimental Morphology says: "We see that organisms respond to light and that this response, exhibited in movements, is not of a widely different order from the disturbances produced in metabolism, which in turn are of the same order as the chemical changes produced in our laboratories upon non-living substances. In a word, response to light is the result of chemical changes in the protoplasm wrought by light." Thus the reaction of these lower organisms is explained as in no way being due to consciousness but as being the result of purely physical causes.

The attempt to mechanize the life of these animals has been definitely formulated in recent years under the physiological theory of tropisms. Loeb (32) has been one of the principal supporters of this theory, also Bethe and Verworn (59) who states that "the phenomena of chemotaxis, thermotaxis, phototaxis and galvanotaxis follow with mechanical necessity as the simple results of differences in biotonus, which are produced by the action of stimuli at two different poles of the free living cell." Holt and Lee (29) also support this theory. One of the chief opponents of this theory as an adequate explanation for a large number of the activities of lower animals in Jennings.

Royce (48) explains what is implied by tropic reactions as follows: " The general character of such reactions is that they determine, in an organism of a given type, a certain characteristic normal position of the organism with reference to its environment, and certain equally characteristic tendencies on the part of the organism to recover its normal position when it is for any reason temporarily lost, and to assume, in the presence of stimuli of certain types, certain directions of movement and certain attitudes which may persist through a great variety of special activities." Jennings quotes Loeb to the effect that " The tropisms are identical for animals and plants. The explanation of them depends first upon the specific irritability of certain elements of the body's surface, and second on the relations of symmetry of the body . . . animals are led without will of their own either toward the source of the stimulus or away from it." As opposed to this view Jennings cites the work of Holmes (27), Miss Smith and others to show that the reaction of certain of the lower animals " indicate adjustments as results of trial and error similar to that employed by higher forms." This is found to be true of the earthworm and of the leech and the larva of the blow-fly.

Harper (21) in a subsequent paper takes up the work of Holmes and concludes that the random movements of an earthworm under light stimulation are of an entirely special character due to causes inherent in its structure.

Radl (44) and Bohn (9) after careful study with various animals find themselves unable to accept the theory of tropisms as an explanation for many of the reactions of the organisms investigated.

Jennings is quite positive that the behavior of lower organisms brings in the factor of consciousness as a necessary explanation for many of their activities.

He believes that the activities of *Paramecium* indicate that they are directed by consciousness. " Still stronger, perhaps, is this impression when observing an amœba obtaining food. The writer is thoroughly convinced, after long study of the behavior of this organism, that if amœba were a large animal, so as to come within the everyday experience of human beings, its behavior would at once call forth the attribution to it of states of pleasure and pain, of hunger, desire and the like, on precisely the same basis as we attribute these things to the dog."

It is evident from the data afforded by the above-mentioned studies that, even in the lowest organisms there are reactions to light and color. Whether these reactions are purely chemical and mechanical or whether with them there is also a conscious element, will perhaps continue to be a debatable problem. In the case of the higher organisms investigated, a conscious basis for their reaction to color and light would hardly be contested. The investigations, however, that have been cited in regard to animals below man have hardly gone farther than to determine the color preferences of the animals experimented with and have not taken up the question of an actual preception or discrimination. Graber, indeed, concluded from his experiments that certain animals at least have a color sensation and perception and that these are more extensive than their color preferences, as manifested by their reaction to color; and also in some instances this perception is finer. However, Graber's experiments if strictly interpreted, can be taken to show hardly more than that many animals find certain intensities and qualities of light pleasurable or unpleasurable. This fact by no means proves conclusively that animals have more than a rudimentary sensitivity in these directions. That they have a clear perception of light and color differences is more or less of an assumption, and the same is true for practically all the other investigations.

Some Recent Studies of the Color Discriminations of Animals.

The definite problem which the present study attempts to present in a preliminary way is not primarily the question of the color or light preference of animals, but of their actual power of perception and discrimination. An animal might well be sensitive to color and light differences but possess no powers of discriminating these or of recognizing them as such.

Until recently no direct work has been done in determining the ability of various animals to actually distinguish colors.

During the last few years, however, a number of studies have appeared which touch upon this problem. Miss Washburn and Bentley (61) have studied the color discriminations of the creek chub. The method used was intended ‘ to test color-discrimination by establishing, if possible, an association between a certain color and food.’ The problem was worked out by two sub-methods, namely: (1) The method of inhibition, and (2) the method of choice. In this first method two colors. a dark red on one pair of forceps and a bright green on the other, were used. A live grasshopper was held in the forceps, only one of which was presented at a time. “ The fish was allowed to take the food from the red forceps; but when it snapped at the green pair, the food was quickly withdrawn.” No constant difference was noticed in the response of the fish to these two colors. The second method was then employed. In this both pairs of forceps were presented at the same time, the red baited and the green empty. After the forceps had been put in position a gate was open and the fish was allowed to swim into a compartment and secure food from the forceps. Several tests were made each day with the red forceps baited and then from one to four tests were made with neither forceps baited. The results were as follows: Red with bait chosen first 169 times; green without bait thirteen times; red chosen without bait forty-two times; green chosen without bait two times. Later blue was substituted and thirty tests were made with the red baited twenty times and ten times unbaited. The red was selected every time in preference to the blue. Later the bait was placed in the green forceps. The results of sixty-nine tests show the green chosen thirty-five times and the red eighteen. The conclusion is reached that the fish “ distinguishes red from green and from blue pigments, the discrimination being independent of the relative brightness of the colors.”

Rouse in his study of the mental life of the pigeon made an investigation of the animal’s color discriminations. His method likewise was “ to investigate the animal’s ability to utilize colors in finding food. . . . Six boxes were used throughout and each was covered with paper of a different color; red, yellow, green, blue (Bradley's standards, except red, RO being substituted), gray and black. The boxes covered with black and gray paper were employed merely to complete the group of six." The results show a positive color discrimination. The proper box was chosen more often than the empty one, but never oftener than the other five combined. The finest discrimination was with green, with a hundred correct choices. The next with red with eighty-eight correct choices. The next with yellow, eighty correct choices. The lowest with blue, seventy-one correct choices. The author further states: "To test the animal's ability to discriminate shades of colors in finding their food, two birds were used, with four boxes, each covered with a different shade of red paper, and two with the boxes covered with green paper. The brightness of the different shades was not measured, but to the eye it seemed to be equal in each of the cases. The food was placed in the box having the most nearly saturated color, and twenty-four trials in series of six, as before, were given each bird. The results were quite similar to those secured with different colors. With the red shades there were twenty-two choices of the best saturated shade to eight, ten and eight, respectively, of the other three; and with green, twenty-one to nine, ten and eight. The forty-three correct choices were distributed from series 1 to series 4 as follows: 7, 11, 12, and 13, which shows learning as before."

Porter (43) tested the color discrimination of the English sparrow and of the cow bird. "Six glasses were covered with the Bradley colored papers, a dark gray, a light gray, a bright yellow, a dark blue, a light green and a dark red." The glass to be chosen contained food. Both birds learned to distinguish colors at about an equal rate. There was by no means a perfect color discrimination. The English sparrow showed some superiority in his discrimination of the blue. The cow bird seems to have been slightly superior in the discrimination of yellow and red. Green was about equally well distributed.

Himstedt and Nagel (26) found that a dog tested by them discriminated between red and blue when several sharply different degrees of brightness were present, and Himstedt in a subsequent test found that the animal could recognize the difference between blue and green and red and green.

Kinneman's study with monkeys included not only an inquiry into their color preferences, as previously stated, but also an investigation of their actual color perception. Kinneman made a large number of tests, nearly seven thousand in all. The brightness of the colors was determined by Rood's 'flicker method.'

The results of the tests the author summarizes as follows:

"1. There can be no doubt that monkeys perceive colors.

"2. Two grays having a given degree of difference in brightness are not discriminated as well as two colors having an equal difference in brightness.

"3. For accurate discrimination of difference in brightness a difference of about thirty-five degrees or none per cent. of the white constituent of the gray is necessary.

"4. The monkeys are able to distinguish colors from grays though the brightnesses are the same.

"5. The male appears to have a preference for bright colors, but blue seems to be discriminated against.

"6. In two instances there were indications of at least a low form of general notion."

In this connection may be also mentioned a statement by Gates (17) in regard to the color discrimination of dogs. He tested these animals with pans of various colors and shades, and with colored metal plates. Under the pans of a given color or shade food was placed and the animals were required to select the proper pan, among many others of different colors and shades. The metal plates were so arranged that certain ones were electrically charged so that the animals received a shock whenever they stepped on these. The writer concludes that the animals tested showed very fine color discrimination. Not a sufficient account is given of the test to judge of its accuracy, or the value of the conclusions reached.

Cole (10) succeeded in training raccoons to distinguish cards of different colors and brightnesses. Considerable difficulty was experienced in teaching the animals to distinguish between blue and yellow and red and green.

Probably the most important contribution to the problem of light and color discrimination in animals is to be found in the recent work of Yerkes (64) on the dancing mouse. The animals were taught to associate two compartments differently illuminated with an electric shock. In the first series of tests the object was to determine whether the animals could distinguish between intensities. It was clearly established that they could do this. In a second series an attempt was made to discover if they possessed color discrimination. In these latter experiments light was filtered through colored screens. Intensities were controlled by the moving backward or forward of electric lamps, their position being measured in reference to the reaction box by a millimeter scale. Brightness was easily regulated, but could not be accurately measured. The only means of comparison was the eye of the experimenter, "and as subjective measurement was unsatisfactory for the purpose of the experiment, no attempt was made to equalize the amount of brightness reduction caused by the several filters." By varying the position of the lamps, a color at one time could be made to appear very much brighter than another and again very much darker, and thus it was sought to eliminate the factor of brightness in judging the ability to discriminate colors.

The writer concludes that "although the dancer does not possess a color sense like ours, it probably discriminates the colors of the red end of the spectrum from those of other regions by difference in the stimulating value of light of different wave-lengths, that such specific stimulating value is radically different in nature from the value of different wave-lengths for the human eye, and that the red of the spectrum has a very low stimulating value for the dancer."

II. Method and Apparatus.

1. *Treatment of Animals.*

The animals used in the present experiment were three dogs, a kitten and a squirrel. A second kitten was also used a few weeks, but as it proved to be unable to learn the simple movements needed in the experiment it was disposed of.

The animals were in all cases given as much liberty as was possible. One dog was allowed his entire freedom in exactly the same manner as he was accustomed to have it. The other two dogs were at first confined in a large well-lighted room, one dog having been placed in the room some time before the experiment began, so that he was perfectly habituated to the room. The second dog was brought in a short time after the experiment was undertaken, but at no time did it seem to be impatient or uncomfortable. Later these two animals were placed in a large and comfortable box stall, well lighted and aired. The kitten was kept in an ample, well-lighted room; it soon became accustomed to its surroundings, although at first it naturally showed somewhat more restlessness than did the dogs. The squirrel lived in a large room in which he had been confined several weeks previous to the beginning of the experiment. This room also was well-lighted and contained a pile of lumber over which the little animal could scramble.

The idea of giving animals as much freedom as was consistent with the experiment was always kept in mind, so as to avoid as much as possible the chief objection often raised against the experimental method of studying animal behavior, namely, that it subjects animals to very abnormal conditions such as close confinement, extreme hunger, etc., and that it does not give animals full opportunity for the display of their intelligence. American psychologists seem to have fallen into this error. The work of Thorndike (55), excellent though it is in introducing the scientific method and in banishing popular observation and anecdote in studying animal intelligence, has been especially criticized in this respect. He confined hungry cats and dogs in narrow cramped boxes from which they could escape to food by learning to operate some simple mechanical devices. It is needless to say that a healthy dog or cat cannot stand such close confinement without becoming worried and excited almost into a frenzy. In the present study the experimenter has often noticed a pronounced dislike of confinement on the part of the animals. It was sometimes necessary to confine the kitten for a minute or two in a small box while the apparatus was being adjusted. Even this temporary captivity was enough to make the animal mew and paw for freedom. Again the kitten was extremely cautions in getting into boxes, even to the extent of refusing to enter a box which contained food. The squirrel, on the contrary, would search the interior of a box for food, keeping, however, a sharp watch. He too disliked captivity as was proved when he was taken out of his usual quarters and placed in a squirrel cage out of doors. He became excited and when released in his accustomed room, hastened to his nest and could not be induced to come down for food for two or three days.

Other American observers, as Kinnaman and Small (51), have experimented upon animals in captivity, but none of them seem to have gone to the extent to which Thorndike did. The work of such psychologists as Morgan and Hill in England and as Mills in Canada, who observed animals out of doors and in their natural environment, offers decided advantages for gaining an insight into the intelligence of animals. If for practical reasons confinement is necessary it should be made as free as possible.

2. *Nature of the Experiment.*

The entire experiment covered a period of about ten months, beginning in October, 1904, and continuing until the following August. Three different sets of experiments were conducted. In the first series all the animals were tested as to their ability to discriminate between a standard red and other colors of various hues and degrees of saturation. In the second series one of the dogs was similarly tested, but with various colors other than red used as a standard. In the third series the attempt was made to determine the animal's ability to abstract the standard red from the particular receptacle on which it had been presented in the first series of experiments, and to recognize it under various and novel conditions.

In the first experiment the method of testing the extent of the color discrimination of the dogs and the squirrel was by allowing them to choose between two painted boxes, both containing equal amounts of food, one of which could be opened and food secured from it, while the other one could not be opened. Two painted pans instead of boxes were used in the experiments with the kitten. A constant color, namely, a saturated red, was chosen for the receptacle which the animal could open, while the color of the second receptacle was varied from time to time. Thus the color red became directly associated with the securing of food and was consequently discriminated from the other colors in proportion as successful efforts to obtain food exceeded expectancy. The boxes and pans, other than red, were changed from time to time in the following order: green, blue, yellow, orange, violet, red-orange, red-red-orange, and red No. 2.

This latter color differed from the standard red by being reduced in saturation approximately ten per cent. In other respects it was the same as the standard. The various pigments used were matched in intensity with the medium gray which stood first in the series of tests. This, of course, did not absolutely prevent a brightness discrimination on the part of the animal tested. The fact that all these colors appeared of equal intensity to the human eye does not necessarily mean that they have the same brightness value for all animals. We know that for color-blind persons the reds and green may have different brightness values than for the normal human eye, and there may be many small degrees of brightness variation even within the range of the 'normal' color-vision of the human eye. Yerkes's experiments with the dancing mouse, as previously stated, seem to show conclusively that the same wave-lengths may possess a decidedly different value for the human and for the animal eye.

The boxes used in working with the dogs were 15 x 10 x 8½ inches. A door 8 x 8 inches was placed on the front of each box. The doors of the red box could be opened when the animals moved a small latch from a horizontal to a vertical position. There was a similar latch upon the inside, the two moving together, thereby keeping the door from being pushed in or pulled out by the animals until the simple act of moving the latch was accomplished. The door was supported by leather hinges and when free, could be either pushed in or pulled out and the animals were then able to obtain the food. The other boxes were exactly like the standard red in size and in every detail except in color. Each of these boxes had a door and a horizontal latch similar in outward appearance to the door and the latch of the red box, but the latch was securely fastened by being nailed, and the door could not be opened.

In experimenting with the kitten a red pan pierced by numerous holes, and 1¾ inches high and 7 inches in diameter at the top, was inverted over a small red wooden platform 14 by 13 by 1¾ inches. This pan had a slightly projecting edge, which rested, since the pan was inverted, immediately upon the platform. Into this projecting edge the kitten learned with considerable skill and nicety of adjustment to insert its claws and to pull the pan a few inches aside until it tipped over the edge of the platform and exposed the food, which had been covered by the pan. A second pan of different color but of equal size and pierced by an equal number of holes and inverted over a similar platform, was rendered immovable by small nails passing through the projecting edge of the pan into the platform below.

The boxes which the squirrel opened were exactly like those presented to the dogs except that they were much smaller being 7¾ x 6½ x 5½ inches with doors 4¾ x 4½ inches. The door of the squirrel's box was placed at the top instead of in front of the box and the animal learned to open it by mounting upon the door, turning the latch and sinking with the falling door into the interior of the box, where he found nuts and fruit awaiting him.

The animals were in all cases given some training in opening the receptacles, as the mere act itself was only incidental to the issue of the experiment. With this assistance all the animals except the second kitten learned to open the receptacles in a few days and the experiment was never greatly delayed on that account. The second kitten failed to learn in several weeks with considerable instruction to remove the pan and perform simple acts which its companion learned in five or six days.

Care was taken throughout the experiment to prevent the animals from associating a certain part of the room with the procuring of food. The boxes or pans were in all cases placed in a part of the room where the light was equal upon both receptacles, but the boxes or pans of a certain color were not placed habitually in one place. They were interchanged on different trials although not always on successive trials.

Another precaution taken was often to replace the standard red box or pan in use with another red receptacle freshly painted. This was done in order to guard against the possibility of the animal determining in any way by the odor which box had been the one most frequently opened. As the introduction of the freshly painted receptacle caused no marked change in the curve, it cannot be supposed that the odor left by the animal's more frequent pawings of the standard red receptacle played any part in the choice.

The conditions of the experiment then were such that all possibilities of a discrimination between the boxes on the basis of odor were excluded; associations with place were eliminated by change of position of the boxes, part of the time the test box being on the right and part of the time on the left. Intensity discrimination was avoided as far as possible by matching the colors in intensity with medium gray. The boxes were all equally and uniformly illuminated by diffused sunlight and the animal when tested approached the receptacles at a place equally distant from each. Under these conditions it seems reasonably certain that all choices were based on an actual color discrimination between the receptacles used.

As has been stated, the standard receptacle (box or pan painted red) was tested with each of the other receptacles (nine in all). After the animal had learned to make the proper discrimination between the standard red and each of the other receptacles, all the receptacles that had been previously used in the experiment were placed in the room and arranged in a semicircle, so as to be equidistant from the animal at the beginning of the test. It should be mentioned in this connection that in all the tests the animal was first introduced to the receptacles at a position some twenty feet removed and at such a point that the various receptacles should be equidistant from this position. Two methods of tabulating the results of the animals' choices were employed. By the former the animal was recorded as having failed in the test, if on being introduced to the receptacles he merely approached or sniffed at the wrong one. By the later method he was credited with a right choice, if he approached the wrong box or pan and then without attempting to open it, went to the right receptacle and opened it, obtaining food. In this second case no error was recorded unless the animal actually attempted to open the wrong receptacle.

III. Results.

1. *Experiment I.*

(*a*) *Dog No. 1.*—The first set of experiments with this animal covered a period of one hundred and fifty-eight days. He was tested with gray, green, blue, yellow, orange, violet, red-orange, and red-red-orange and red No. 2 in the order named and followed by all the boxes. After the first week he had learned well the discrimination for gray, and from that time on the curve drawn by the first method never fell below expectancy for the entire period this box was presented. On four occasions it touched fifty per cent., but on the whole was clearly above expectancy.

When green was introduced the curve fell somewhat for two days, but quickly recovered. The lowest point reached with this color was fifty-nine per cent. On four days it was at one hundred per cent.

Blue also shows a high average. It fell slightly when this color was first introduced, but immediately rose to one hundred per cent., where it remained for the majority of the trials. With yellow the results were not so high. Once with this color the curve fell below expectancy, but altogether maintained a high average.

The test with orange was always above expectancy, and the curve continued to mount through violet, red-orange and red-red-orange.

On the introduction of the red just observably different from the standard, there was a fall to fifty per cent. for two succeeding days, then a rise to fifty-seven per cent., a fall to fifty-six, a rise to sixty-four, a fall to fifty, a rise to one hundred, a fall to fifty-five and then to fifty, followed by a rise to one hundred and a fall to fifty.

When all the boxes were employed simultaneously the animal at first was clearly confused and the curve fell, yet never went as low as expectancy (ten per cent.). The lowest point touched in this last test was forty-two per cent., the highest one hundred, which was reached on the last two days of this test.

When the results were computed by the second method the curve was still higher. Not one failure was recorded for blue, the average throughout being one hundred. Only two failures were recorded for yellow, one for orange, one for violet, none for red-orange, one for red-red-orange, three for red just observably different, while with this color there were seven days on which the hundred mark was reached. With gray and green the results were high above expectancy, but considerably lower than with the succeeding colors.

The results of the above tests expressed by both methods in per cents with mean variation for each average follow:

	Av. 1.	M. V.	Av. 2.	M. V.
Gray	66.7	14.4	81.7	14.9
Green	84.9	11.0	89.0	10.0
Blue	93.1	11.7	100.0	0.0
Yellow	84.4	17.4	91.0	17.1
Orange	77.5	14.9	98.3	2.7
Violet	87.8	13.3	97.2	5.2
Red-orange	91.4	9.7	100.0	0.0
Red-red-orange	93.4	11.8	98.4	2.8
Red No. 2	64.4	14.9	81.3	18.1
All	67.8	17.4	67.8	18.4
Average	81.1	13.6	90.5	8.9

(*b*) *Dog No. 2.*—The first experiment with this animal occupied one hundred and sixteen days. The colors were presented in the following order: green, blue, yellow, orange, violet, red-orange, red-red-orange, and red No. 2, followed by all the boxes. The results computed by the first method show this animal to have been inferior in accuracy of discrimination to dog No. 1; but the curve is clearly above expectancy. The first three days showed fifty per cent. of correct choices, then the curve rose to one hundred per cent., but soon fell again to fifty per cent., mounted to sixty-six, then fell to fifty-seven, where it remained for two days.

At this point blue was introduced and the curve fell to fifty-four per cent., then to fifty-three, then to fifty. After this it gradually rose and for some days remained at one hundred per cent.; it then fell again, rose and fell once more, but finished high.

On the introduction of yellow the curve fell to sixty-one per cent., but then, after some fluctuation, rose to one hundred, where it continued for six days. It soon fell to fifty but rose, fluctuated and finished relatively high.

Orange when introduced caused a fall to sixty per cent., followed by a rise to one hundred; later a fall which reached as low as sixty-three per cent., followed by a number of perfect choices and then a fall to seventy-one per cent.

Violet at first caused a rise to one hundred per cent., with a later fall to sixty-seven per cent. for several days and a subsequent return to one hundred per cent., with a later fall to eighty-seven per cent.

When red-orange was introduced there was a fall of sixty-seven per cent., then a rise to one hundred, a second drop to seventy-five per cent. and a recovery to one hundred per cent.

Red-red-orange caused a fall to seventy-eight per cent.. After this there was a rise to one hundred per cent., followed by an abrupt drop to fifty per cent. Then the curve fluctuated with a maximum at eighty-eight per cent. and a minimum at sixty-seven. It ended at eighty-three per cent.

With the introduction of the red just observably different from the standard there was a fall to fifty per cent., a rise to sixty-seven and a fall to thirty-eight, then marked fluctuations, ranging from eighty per cent. to twenty-five per cent. The test ended with the curve at fifty per cent. On the introduction of all the boxes the curve fell to forty-seven per cent. It then rose to sixty-eight per cent. and at once fell to eighteen per cent. There was, however, an immediate recovery with a final rise to one hundred per cent. The test ended at eighty per cent.

The tabulation by the second method showed a considerably higher average throughout. This was very marked with green and yellow, where the number of successful trials was far in excess. With blue the lowest point reached was sixty per cent., the next lowest seventy-one. There were several days when the curve stood at eighty per cent. Once it stood at eighty-nine per cent. and the later days of the experiment without fluctuations at a hundred.

With yellow the lowest point reached was fifty per cent. With this exception during the test with this color the curve remained at one hundred.

The average with orange was also high. The lowest point reached was sixty-three per cent.; there were several at seventy-three, one at eighty and the remainder at one hundred per cent.

Violet fell on several occasions to sixty-seven per cent. Otherwise the curve stood at one hundred.

Red-orange showed no failures, but red-red-orange fell once to fifty per cent. The next lowest point recorded was sixty-seven. For the remainder of the time the curve fluctuated between eighty-three and one hundred per cent.

With red No. 2 there were wide fluctuations. At one time the curve fell to forty per cent. and again to forty-five. There was, however, one day at seventy-five and several days at one hundred.

The lowest point reached with all the boxes was eighteen per cent., the highest was one hundred. Between these extremes the curve fluctuated from forty-seven to eighty per cent.

The summary of averages and mean variations by both methods follows:

	Av. 1.	M. V.	Av. 2.	M. V.
Green	67.7	12.6	79.6	18.2
Blue	79.6	18.4	85.3	12.9
Yellow	87.9	13.7	97.6	4.6
Orange	82.8	14.7	91.0	11.9
Violet	85.3	13.4	92.4	11.6
Red-orange	87.0	13.0	100.0	0.0
Red-red-orange	77.0	11.8	87.6	13.8
Red No. 2	54.8	15.7	84.2	22.7
All	64.2	16.8	65.3	16.4
Average	76.3	14.4	87.0	12.3

(*c*) *Dog No. 3.*—This animal showed less decision than the two previously tested. He often spent much time in running from one box to the other with many sniffings, but with only an occasional attempt to actually open either box. This lack of purpose is largely to be accounted for by the fact that, unlike the other animals, he was permitted entire freedom and was not so hungry on most occasions as were the other animals. There is a marked difference shown between the two curves plotted for the trials of this animal.

This animal was tested with the colors introduced in the following order: gray, green, blue, yellow, orange, violet, red-orange, red-red-orange and red No. 2, followed by all the boxes. The results showed less success in the case of this animal than with the first two dogs, especially when plotted by the first method.

The curve plotted by the first method showed comparatively little discrimination for either gray or green, the first two test stimuli introduced. However, twice during the test for the former the curve reached one hundred per cent. It fell below expectancy, however, on several occasions, going as low as zero. For the majority of the tests it fluctuated between fifty per cent. and eighty-seven.

With green the highest point reached was eighty-seven per cent. Once it fell to zero, and on three occasions registered fifty per cent. It varied then from fifty-eight to eighty-one per cent.

The tests with blue gave more satisfactory results. On four occasions the curve reached one hundred per cent., while twice it sank as low as fifty. For the remainder of the time it varied between fifty-seven and eighty-nine per cent.

The test with yellow was somewhat less satisfactory. Although the curve never fell below expectancy during the time that this color was used, it reached fifty per cent. on six occasions. The highest point reached was eighty-nine per cent.

Orange brought the curve down on the first day from eighty-four to fifty per cent. On the third day it sank to twenty-five per cent. During most of the test it fluctuated between fifty-seven and eighty-six per cent.

The results with violet were negative, the curve falling below expectancy on three occasions, and maintaining a low level throughout.

On the introduction of red-orange there was a decided rise in the curve. Only on one occasion did it fall below expectancy, and only once again did it reach as low as fifty per cent. It stood twice at one hundred, and for a considerable part of the time fluctuated between sixty-seven and eighty-seven per cent.

Red-red-orange showed negative results and red No. 2 was but slightly better discriminated, the curve for this color varying from sixty-seven to thirty-five per cent.

Red was fairly well discriminated from all the colors when they were introduced together, the average being decidedly above expectancy.

The curve plotted by the second method showed little ability to discriminate during the first eight days of trial. After this, the curve rose above expectancy on three occasions, reaching one hundred per cent., and the remainder of the time during the test with gray, fluctuated between sixty-seven and eighty-one per cent. The test with green showed better color discrimination, the lowest record being fifty per cent., on the first day. During the remainder of the experiment with this color the curve was between sixty and eighty-eight per cent., except on three occasions when it reached one hundred. The discrimination for blue was relatively high; on eleven occasions the curve reached one hundred per cent., and fell to fifty only once; the remainder of the test it varied from sixty to eighty-nine per cent. The introduction of yellow caused the curve to fall from one hundred to fifty-nine per cent. Twice during this part of the test it fell to fifty per cent.; on the other hand, on nine days it reached one hundred per cent. The remainder of the time it varied from sixty to ninety per cent.

The introduction of orange showed a rise to one hundred per cent. The curve, during this test, was high, except for one day when it fell to zero. On nine days it stood at one hundred. The next highest point was eighty-six per cent., the lowest with the exception of the fall to zero was seventy-one per cent.

The results with violet were for some reason practically negative. On two occasions the curve reached zero, and once again it fell as low as twenty-five per cent. Only on two days did it reach the hundred mark.

Red-orange, on the other hand, shows a very clear discrimination. On seventeen days the curve stood at one hundred. Once it fell to zero, however. The remainder of the test it varied between fifty and sixty-seven per cent.

The test with red-red-orange was equally satisfactory. On eight days it was at one hundred, while the lowest point reached was eighty per cent.

Red just observably different was fairly well discriminated. On one occasion the curve sank to forty per cent., on another to fifty. With these two exceptions it was well above expectancy, standing at one hundred on three days.

Upon the introduction of all the boxes the curve fell from one hundred per cent. to sixty, then to fifty. Later it went up to one hundred where it was found on two occasions. Once, however, it sank to zero. The next lowest was at expectancy, or ten per cent. For the remainder of the test it varied between fifty and seventy-five per cent. The averages for both methods follow:

	Av. I.	M. V.	Av. II.	M. V.
Gray	64.5	20.2	74.8	3.9
Green	62.2	15.6	80.7	13.6
Blue	76.3	12.7	89.5	12.3
Yellow	59.2	12.4	85.0	16.5
Orange	62.0	16.8	86.0	11.1
Violet	48.2	9.3	51.7	8.9
Red-orange	70.3	11.9	89.7	10.9
Red-red-orange	44.7	28.6	97.5	17.5
Red No. 2	53.0	7.0	74.1	18.6
All	58.6	29.1	59.2	28.7
Average	59.9	16.4	78.8	14.2

(*d*) *The Kitten.*—The kitten was tested with the colors in the same order as that presented to dog No. 3.

After the first week of tests the gray was clearly discriminated. The curve plotted by the first method stood on eight days at one hundred per cent. On one occasion, however, it fell as low as twenty-five per cent., on another to forty-five.

For the most part, however, it fluctuated between fifty-five and seventy-five per cent.

Green reached fifty per cent. but once and never went below that point. On the other hand, however, it rose to one hundred per cent. but twice during the entire experiment with this color.

Blue was on the whole higher, reaching one hundred six times. Once, however, it fell to forty per cent. During a considerable part of the time it fluctuated between sixty and eighty-five per cent.

The discrimination for yellow proved to be more successful than for any of the previous colors presented. On fourteen occasions the curve was found at one hundred per cent. Twice it fell to fifty, while the remainder of the time it varied between sixty-three and eighty per cent.

Orange showed a relatively high average, standing at one hundred per cent. during slightly more than half the test. Once, however, it fell to fifty per cent. The next lowest point was seventy-five per cent.

The test for violet gave less favorable results. The highest point reached by the curve was eighty per cent. The lowest was thirty-eight per cent. On two occasions the curve stood at fifty per cent., and the remainder of the test found it between fifty-seven and seventy-five per cent.

Red-orange gave higher average results. The curve on one occasion reached one hundred per cent. The next highest point was eighty per cent.; the lowest was seventy-seven per cent.

During the test with red-red-orange, the curve varied between forty-five and seventy-eight per cent. For about half the tests it was either at fifty per cent. or slightly above or below.

With the red No. 2 the curve was similar to that for red-red-orange. Once, however, it rose to one hundred per cent. The lowest point reached was forty per cent.

The introduction of all the boxes resulted in total failure on the first day of the test. During the rest of the time the curve stood above expectancy. For nearly half the tests it stood at one hundred.

The results of the tests of the kitten as tabulated by the second method show a considerably higher average throughout. Only on four occasions during the entire test did the curve fall below expectancy, once in the case of the gray, once with red, once with green and once when all the boxes were presented. The results with the gray were satisfactory throughout. On sixteen days the curve stood at one hundred, and only on one day did it fall below sixty-eight per cent.

Sixty per cent. of the tests with green resulted in a perfect discrimination. On one occasion, as previously stated, the curve fell to zero. The next lowest point was sixty per cent.

Seventy-five per cent. of the tests with blue found the curve at one hundred per cent. The lowest point reached in this test was sixty-eight per cent.

For yellow the number of perfectly successful tests was still higher, being eighty-one per cent. of the total number. Once the curve fell to expectancy. The next lowest point reached was eighty per cent.

Orange was without a failure, but violet showed a decided drop, although the lowest point reached during the tests with this color was sixty per cent. Approximately one third of the tests with this color were completely successful.

Red-orange was almost perfectly discriminated. Only once did the curve fall below one hundred per cent.

The tests with red-red-orange showed a slight fall in the averages, although the curve never fell below expectancy. It stood on one day at fifty per cent. The next lowest point was seventy-two per cent. In more than two thirds of the trials a perfect discrimination was shown.

On the introduction of the red just observably different the curve fell from one hundred to forty per cent. It then gradually mounted until it reached one hundred per cent. It then fell to eighty per cent. but rose once more to a hundred. On the last two days of the test it fell again and reached thirty-three per cent.

On the introduction of all the boxes it fell to zero, and then rapidly recovered, reaching one hundred per cent. where it remained throughout the test.

The averages of the results of the tests tabulated by both methods together with the mean variation are as follows:

	Av. 1.	M. V. 1.	Av. 2.	N. V. 2.
Gray	69.2	18.2	87.8	15.6
Green	69.9	11.6	82.3	15.0
Blue	79.3	20.3	92.2	11.4
Yellow	86.6	15.0	91.6	9.4
Orange	83.0	17.0	100.0	0.0
Violet	59.4	12.0	81.4	15.9
Red-orange	76.1	7.0	98.3	3.0
Red-red-orange	54.7	8.5	90.1	12.0
Red No. 2	58.4	12.4	65.0	18.2
All	63.8	24.7	65.0	21.2
Average	70.0	14.7	85.4	12.2

(*e*) *The Squirrel.*—The tests with the squirrel were begun somewhat later than those with the other animals, and covered a period of twenty-two weeks, exclusive of a break of one month during which the tests were suspended. The animal soon learned to discriminate with fair accuracy gray from the standard red, the curve fluctuating between expectancy and eighty per cent.

The test with blue showed increasing ability in discrimination. The highest point reached was one hundred per cent. on two days. The curve was found at expectancy on three days. The remainder of the tests resulted in a variation of discrimination between fifty-four and ninety per cent. Once during the test with this color the squirrel made no attempt to open the box and a break in the curve resulted. In subsequent tests this failure to respond was often to be noted, and there were a considerable number of breaks in the curve for this reason.

The introduction of yellow caused the curve to fall from one hundred to fifty-five. It then rose to sixty-five and fell to forty-five. A recovery followed, and then another fall from one hundred to forty-five, followed by a subsequent rise to one hundred and a fluctuation during the remainder of the test between that point and sixty-five per cent.

On the introduction of green the curve again fell, from one hundred to sixty-two per cent. A slight rise then followed and a subsequent fall to twenty-three per cent. Then came a rise to sixty-three followed by a slight fall and a break.

When the orange box was substituted for the green the curve mounted to one hundred. There it remained for three days and later fell to expectancy. A recovery to the highest point soon followed. The remainder of the test showed the curve relatively high, the lowest point reached being sixty per cent.

The introduction of violet was followed by a rise to one hundred per cent., and then a fall to sixty, the lowest point reached until the last day of the test for this color, when the curve was found at fifty per cent.

Red-orange furnished a series of high averages. The lowest point reached being seventy-five per cent., the highest one hundred. One break was found in the curve during the test with this color.

The red-red-orange brought the curve down to sixty-two per cent.; it then rose to seventy-eight per cent., but soon fell to expectancy. For some days it varied between expectancy and eighty-four per cent. It then sank to forty-six per cent., recovered again, but some days later fell to twenty-five per cent. Later there was a break, after which it rose to one hundred per cent. It soon fell again to fifty per cent., and then for some days maintained a relatively high average, reaching one hundred per cent. on several occasions. The break of one month resulted in a material lowering of the curve. It soon fell to zero, then recovered and fell again to the same point. The next lowest point reached was twenty-five per cent. During the last few days of the test it stood at one hundred per cent.

On the introduction of red No. 2 the curve fell to expectancy. It recovered; then sank to zero. It subsequently rose to one hundred and during the remainder of the test never fell below sixty-six. During the last five days of the test the curve stood at one hundred.

Tabulation by the second method shows a much higher average of discrimination. This was not so striking with the gray as with the subsequent tests. With blue the curve fell no lower than sixty per cent. For the great majority of the tests the discrimination was perfect, and for over two weeks during the latter part of the test with this color the curve was constantly at one hundred. During the first week with the tests with yellow the curve ran low. Later it reached one hundred, where it remained until the end of the test, except on one day when it fell to sixty-four per cent.

For green the average was very much higher by the second method, the lowest point recorded being eighty-six per cent. For much of the time the curve stood at one hundred.

For orange the curve ran equally high, also for violet and for red-orange.

The introduction of red-red-orange brought a lowering of the curve to eighty; then after several days of entirely successful discriminations there was a fall to sixty per cent. Then, except for an occasional drop, the curve remained for many successive trials at one hundred. The break of one month resulted in an immediate fall of the curve to forty-two per cent., and a later fall to twenty-five per cent., followed by an ultimate rise to one hundred per cent. where it remained for some days.

Red No. 2 caused a fall to expectancy, where it remained for several days. It soon recovered, however, and during the last days of the test stood at one hundred per cent.

The table of averages for each color tabulated by both methods follows:

	Av. I.	M. V.	Av. II.	M. V.
Gray	67.0	12.4	81.3	3.5
Blue	72.2	11.6	98.4	2.1
Yellow	79.5	16.5	91.9	13.0
Green	65.8	11.6	99.0	1.9
Orange	85.8	14.9	99.1	1.7
Violet	75.9	10.7	97.5	3.6
Red-orange	87.0	12.1	98.9	1.9
Red-red-orange	78.9[5]	16.8	90.8[5]	13.2
Red No. 2	(56.6)[6]	(22.3)	(61.1)[6]	(15.7)
Total	65.6	21.8	69.6	15.0
Average	75.3	14.3	91.8	6.2

[5] Before break.

[6] After break, not averaged with other results.

2. *Experiment II.*

A second series of tests were made with dog No. 2 in which blue, yellow and green were used as standards, and the various colors were introduced for comparison. This experiment was brief, occupying but nine weeks. The results show that yellow was most successfully differentiated from these three standard colors. After preliminary tests no failures were made with this color as a standard. The results with blue and green were identical, the average for each color when used as a standard being ninety per cent.

3. *Experiment III.*

In this experiment, as has been previously stated, the attempt was made to determine whether the animals had the power to associate the standard red with the procuring of food under various novel conditions. The aim, in other words, was to discover whether the animal could abstract the color from the receptacle with which he had experienced it in the first series of tests, and recognize its meaning when it was presented to him on receptacles of various sizes and shapes and under diverse circumstances. The three dogs and the kitten were submitted to this test.

The tests with all these animals with the exception of dog No. 2 were relatively brief. Dog No. 1 was tested for fifty-four days and the kitten for seventy days. The tests with dog No. 2 covered a period of one hundred and seventeen days, exclusive of a break in the latter part of the experiment of twenty-three days during which time these tests were discontinued.

In the case of the first three animals tested in this manner, pans, boxes and receptacles of varying shapes were introduced with the stimulus color painted on them; and other receptacles of diverse forms and colors were likewise presented. The combination was varied from day to day, so that no association based on general form could be set up. Typical receptacles thus used are described below:

A red rectangular box 7⅝ x 6¼ x 5¼ inches, with a lid which the animals could easily knock off.

An ordinary cuspidor painted blue, 3½ inches high and 7 inches in diameter, inverted upon a blue platform.

A red oval pan 10¼ inches longest and 7¾ inches shortest diameter; the top of this pan was pierced with holes and it was inverted on a platform used in the experiments with the kitten.

A red strainer used in kitchen sinks, which was inverted over a red platform; this strainer was, when inverted, 4½ inches high at the highest point, with a rounded front incline.

An ordinary milk strainer painted red, 2⅞ inches high, 8 inches in longest diameter at the bottom when inverted over a red platform, and 4 inches across the top.

A red triangular box 4½ inches high, 4½ inches deep and 8½ inches long at the base, resting upon a red platform; this box was tightly closed at the back and across the front was a door extending to within an inch of the apex of the box; this door would fall when attacked by the animals and the whole contrivance could be pushed aside and food secured.

A red box 5 inches wide and 1¾ inches deep, of general rectangular shape but with one end extended into a triangle; one side of this rectangle was 9½ inches long, the other was 8¼ inches and the box at the apex of the triangle was 11½ inches long.

A red box, consisting of two rectangles joined at right angles and communicating into each other. This box was 3½ inches high; the rectangles were 10½ inches in length upon the longer side, and 9½ inches respectively upon the shorter side. This box was fitted with a lid which could easily be knocked off.

A red box with lid in the shape of a triangle, 8½ x 7½ x 8½.

A red cup inverted upon a red platform.

A red box, generally rectangular in shape, but with a square gable placed in the center of the rectangle and communicating with it. This box was 9 inches long, 2½ inches wide and 3 inches high except the middle portion which was 5 inches high.

A long rectangular box painted red, 11½ inches long, 3 inches wide and 1¾ inches deep. This box had a lid which could be easily knocked off and food secured.

A red pan 2 inches high and 8 inches in diameter, notched irregularly and with a hole 1 3/4 inches in diameter in the center of it. When this receptacle was used with the dogs a small red wooden block covered this hole; when the apparatus was used with the kitten, this block was wanting.

During the latter part of the more extended experiments with dog No. 2 the conditions were made more complex. The animals were tested at times with boxes painted with gray or other colors and with only a stripe or circle or cross of the stimulus red; again a red flag of the stimulus color was attached to various receptacles, and on several occasions a red cloth thrown over the food was used as a test. The results in every instance were calculated by the second method, trials being counted only when the animal made a clear attempt to open one of the receptacles, chance sniffings and wanderings being ignored.

A description of the results of these experiments with the various animals follows:

Dog No. 1.—For the first thirteen days of the tests with this animal little discrimination was shown, the average being 65.2 per cent. of correct trials. During the remainder of the test, the curve fell below one hundred per cent. only on six occasions, five of these times reaching fifty per cent. and once thirty-three per cent. During the last ten days of this series not a failure was recorded.

The tests with dog No. 3 were not satisfactory from the fact that it proved even more difficult than in the first series to get reactions of any sort from this animal. During the twenty-four days of this test the animal failed to respond at all on ten. Although he showed evidences of discrimination when reactions were secured, the results were too irregular and trials with him were discontinued.

The kitten learned with much more difficulty to react successfully to this set of test than did dog No. 1. For the first forty-six days out of the total of seventy, the curve shows only a fair degree of discrimination, the average being 74.6 per cent. For the remainder of the series the results are much better. There were several days on which the animal made no attempt, but otherwise the results were uniformly high, there being only one failure recorded, when the curve fell to expectancy. During the last week not a failure was recorded.

It is evident from these results that both dog No. 1 and the kitten learned to make the rudimentary abstractions required to correctly perform the test.

Dog No. 2.—During the first part of the period the animal was given the more simple tests, similar to those used with dog No. 1 and with the kitten; during the last thirty-one days he was tested under more difficult conditions. As before stated during this latter part of the test he was required to distinguish the standard color when it appeared only as a band of color, or as a cross or a flag attached to a receptacle or as a cloth thrown over the food, or under similar complex conditions. His reactions fall into five fairly distinct periods as follows:

For the first eighteen days he was learning to adjust himself to the novel conditions of the test. During this time the curve fell below expectancy on seven days. The average for this period was 58.7 per cent. of correct trials.

For the succeeding sixteen days he had reached a level of very constant perfection. During this time he failed only twice to choose perfectly. His average for this period was 94.7 per cent.

The third period, for the next forty days, showed only one day on which his discriminations were not perfect. On that day for some reason he failed entirely. Otherwise the curve stood uniformly at one hundred per cent.—his average for the entire time being 97.5 per cent.

At this point the experiments were discontinued for twenty-three days, and then resumed. On the first day after this break the dog's reaction showed just fifty per cent. of right choices, but for the following eleven days there were no failures, making the average for these twelve days 95.8 per cent.

The fifth period covers the time of the more complicated experiments. The results for the first eleven days showed a marked falling off from the discrimination of the preceding tests. However, the curve fell below expectancy but once, when it sank to twenty-five per cent. On three days it was at exactly fifty per cent., giving a total average of 63.3 per cent. The remaining twenty days showed better results. On fourteen of these there were no failures and though the curve fell to fifty per cent. on several occasions it never went below expectancy. The average for this entire period of thirty-one days was 70.7 per cent., showing a clear discrimination though with considerably more failures recorded than during the preceding tests under more simple conditions.

IV. General Conclusions.

1. *Accuracy of Discrimination with Various Colors.*

In considering the results of the first two tests the question arises, whether any of the colors were more accurately and uniformly discriminated from the standard red than were others, and if so what was the basis of this discrimination. Theoretically two opposing possibilities seem open, namely: (1) In associating red with the obtaining of food the animal might practically ignore the other colors presented, and thus not discriminate between them and the stimulus color until they so closely approached the standard as to cause actual confusion, or (2) on the other hand the colors discriminated against might enter clearly into the consciousness of the animal and he then would make successful trials in proportion as their difference from the standard was evident to him. If the first of these alternatives were the correct explanation, we should expect to find that the colors farthest removed from the red would be the ones with which the greatest number of correct choices were to be found. This, however, as the following figures will show, was not the case. The manner of the choice of the various animals should also indicate something in this connection. If the consciousness of the animal were entirely, or for the most part, centered on the stimulus color, to the exclusion of the others it would be likely that in making his choice he would go directly to the proper box, and completely ignore the others. This was not true in the case of the three dogs and the kitten at least, even in successful trials. It often took several minutes of inspection, during which the animal walked in front of the receptacles, before he actually made an attempt to open one of them and secure the food within. He gave the appearance of actually deliberating or 'making up his mind,' and though no elaborate process of comparison should be attributed to his consciousness at this time, yet all his actions indicated that the color opposed to the stimulus red was sufficiently an element of consciousness to make the choice of the proper receptacle difficult until the proper color discrimination had been made. With the squirrel there appeared to be less of this seeming deliberation than with the other animals tested. Further, if the color to be discriminated against entered clearly into the perception of the animal it would be expected that on the introduction of that color for the first time the curve would tend temporarily to fall. This was the case in general. In seventy per cent. of all the tests there is such a fall. In one per cent. there is neither rise nor fall and in twenty-nine per cent. a rise which, however, is in almost every instance soon followed by a fall below the general level at which the curve stood at the end of the test with the preceding color. As a rule the first few days of the trials with a newly introduced color show less accuracy of discrimination than do the succeeding days of trial with that color. The following table gives the relative success of each animal in discriminating the stimulus color from the others.

TABLE GIVING GENERAL SUMMARY OF RESULTS.

(Figures at right of column indicate per cents by first method, figures at left per cents by second.)

	Green.	Blue.	Yellow.	Orange.
Dog 1	84.9–89.0	93.1–100.0	84.4–91.0	77.5– 98.3
Dog 2	67.7–79.6	79.6– 85.3	87.9–97.6	82.8– 91.0
Dog 3	62.2–80.7	76.3– 89.5	59.2–85.0	62.0– 86.0
Kitten	69.9–82.3	79.3– 92.2	86.6–91.6	83.0–100.0
Squirrel	65.8–99.0	72.2– 98.4	79.5–91.9	85.8– 99.1
Average	70.1–86.1	80.1– 93.1	79.5–91.4	78.2– 94.9

	Violet.	Red Or.	R. R. Or.	Red 2.
Dog 1	87.8–97.2	91.4–100.0	93.4–98.4	64.4– 81.3
Dog 2	85.3–92.4	87.0–100.0	77.0–87.6	54.8– 84.2
Dog 3	48.2–51.7	70.3– 89.7	44.7–97.5	53.0– 74.1
Kitten	59.4–81.4	76.1– 98.3	54.7–90.1	58.4– 65.0
Squirrel	75.9–97.5	87.0– 98.9	78.9–90.8	65.6– 69.6
Average	71.3–84.0	82.3– 97.4	69.7–92.9	59.2– 74.8

In the case of dog No. 1 the color having the highest average of correct choices, according to the first method of tabulation, is red-red-orange. The other colors follow in the order named—blue, red-orange, violet, green, yellow, orange and red No. 2. According to the second method of tabulation the order is, red-orange and blue (both at one hundred per cent.), red-red-orange, orange, violet, yellow, green and red No. 2. Although no pronounced color preference is here indicated there seems to be a tendency toward the red-oranges and the blue and away from the green and the yellow.

With dog No. 2, the table, according to the first method of calculation, shows a preference for yellow, with red-orange second, and the remaining colors in the following order, namely—violet, orange, blue, red-red-orange, green and red No. 2. The results according to the second method of tabulation are—red-orange, yellow, violet, orange, red-red-orange, blue, red No. 2 and green. This animal shows a preference for the orange, the yellow and the red-orange as against blue and green.

Dog No. 3 reacted most accurately to red when contrasted with blue, according to the first method of tabulation. The color next highest is red-orange, then green, orange, yellow, red No. 2, violet and red-red-orange. The second method gives the order as follows: red-red-orange, red-orange, blue, orange, yellow, green, red No. 2, violet. This indicates a clearer discrimination for blue and the red-oranges.

The order of discrimination for the kitten by the first method is—yellow, orange, blue, red-orange, green, violet, red No. 2, red-red-orange; by the second method—orange, red-orange, blue, yellow, red-red-orange, green, violet, red No. 2. Here a preference is indicated for yellow the red-oranges and blue.

The order for the squirrel is, by the first method—red-orange, orange, yellow, red-red-orange, violet, blue, green, red No. 2; by the second method—orange, green, red-orange, blue, violet, yellow, red-red-orange, red No. 2. The oranges on the whole seem to stand highest.

An average of the results for all the animals gives the order tabulated by the first method as follows: red-orange, blue, yellow, orange, violet, green, red-red-orange, red No. 2; by the second method the order is—red-orange, orange, blue, red-red-orange, yellow, green, violet, red No. 2.

Summarizing these results in another way it may be seen that the colors standing highest were as follows: red-orange (3), blue (2), yellow (2), orange (2), red-red-orange (2). The two lowest colors were violet (2) and green (4). From this comparison red No. 2 is omitted because of the fact that its nearness to the standard made accurate discrimination difficult, it being the lowest color in six instances, green standing below red No. 2 twice, and violet twice.

By glancing at the above table it will be further seen that the colors averaging above ninety according to the first method of tabulation were: blue (1), red-orange (1), red-red-orange (1); according to the second method—yellow (4), orange (4), red-orange (4), red-red-orange (4), blue (3), violet (3), green (1). The colors averaging below seventy according to the first method of tabulation were—red No. 2 (5), green (4), violet (2), red-red-orange (2), yellow (1), orange (1); according to the second method—red No. 2 (3), violet (1).

The above facts taken together do not show any pronounced discrimination in favor of one color. On the whole the various oranges and the blue stand the highest, while the violet and the green, together with red No. 2, occupy the opposite end of the scale.

It should, however, be noted in this connection that the temporal order of green, since it came early in the series, may have had something to do with the lower record made with this color than with those coming later. This is particularly true in the tests with dog No. 2, in whose case, because of the exigencies of the experiment, green was introduced before he had clearly learned the proper reaction to the stimulus color with gray. For this reason, probably, green should be placed somewhat higher in the series, perhaps above violet.

These results seem quite the reverse of those that Rouse found to be true of his pigeons. With them the finest discrimination was with green and the lowest with blue. Kinneman found that with his monkeys blue was discriminated against. The results are more in line with the tests made in color discrimination in children, although Preyer places blue last with green next. Binet's order as previously stated is red, blue, orange, rose violet, green, yellow. Baldwin's tests placed blue first with green next to the last, and higher than brown; while Nagel places green, violet and red on an equal footing and assigns to blue the lowest place in the list.

Rivers (47) in an investigation of the vision of the natives of the Torres Straits has made an extensive study of their color vision. Among other things he concludes that the "color vision of the Papuan is characterized by a certain degree of insensitiveness to blue (and probably green) as compared with that of Europeans." His reasons for this conclusion are: (1) The races examined had either no word for blue or an indefinite one. Blue is probably for them a darker and duller color than for us. (2) The Holmgren test showed that blue and green were constantly confused. (3) Observations with the tintometer showed that "the Murray Island natives distinguished red when very faint, much more readily than blue." Rivers found that the savages tested by him showed a much finer sensibility for red than do Europeans. The investigator adds that this lack of discrimination for blue is in line with the fact that "in ancient literature, as among modern barbarous and savage races, it is the color blue for which nomenclature is especially defective. . . . The color vision of the Torres Straits islander gives some support to the views of Gladstone, Geiger and Magnus that the defective color language of ancient literature may have been associated with a defective color sense."

Investigations by Seligmann[7] reported in an appendix to the study of Rivers, on the vision of natives of British New Guinea show confusion and uncertainty in color nomenclature, particularly in regard to blue, green, indigo, violet and black. Later investigations by Rivers point to the same conclusions as his earlier experiments. The lack of discrimination for blue shown in these anthropological studies finds no parallel in the tests of the present experiment.

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[7] C. G. Seligmann, 'Cambridge Anthropological Expedition to the Torres Straits,' Vol. II., Pt. I.

2. *Fineness of Color Discrimination.*

The tests seem clearly to show a surprising fineness of color discrimination among the animals tested. The averages show that the red-orange was more successfully discriminated from the standard red than was the blue, yellow, green and violet, although the red-orange is nearer in quality to the standard than any of these. Even red-red-orange, according to the second method of tabulation, stands above yellow, violet and green and is practically on a par with blue. It is only when the red which is barely observably different from the standard is introduced that there is a marked lowering of the curve. Even here the successful attempts (according to the second method of tabulation) are three fourths of the whole.

3. *Memory for Color of the Animals Tested.*

Incidentally in the course of the experiment two of the animals, namely, dog No. 2 and the squirrel were tested for their ability to remember the standard color after some little time had elapsed since they were last tested with it. These memory tests were not originally planned, but were made merely because of the exigencies of the experiment. The course of the experiment, as previously stated, was suspended for three weeks in the case of dog No. 3 and practically no loss of memory for the standard color was shown.

Doubtless a much longer period might have elapsed without substantial loss—in the case of the squirrel the effect was different. After one month's suspension of the trials with red-red-orange the experiment with the squirrel showed for the first few days after renewal of the tests practically no discrimination. Although the results later were somewhat better the animal showed for a considerable time the effect of the suspension of the tests.

4. *Ability to Form Abstract Notions in Regard to Color.*

The most significant part of the experiment with the various animals was that which was conducted under the third series of tests, since the results here seem to point to a somewhat higher mental capacity among the animals than that ordinarily assigned to them. There has been considerable discussion in recent years as to the power of animals to abstract from the flow of their concrete experience any elements which shall be of general significance in the guidance of their conduct. Thorndike in particular feels sure that animals do not have the power to form 'free ideas,' as he terms them. Their acts are due to simple associations learned in a definite series; and they show no ability of applying the data of past experiences to novel situations. On the other hand, we find some writers on animal intelligence who attribute much higher mental powers to brutes. Ramsey (45) asserts that under a sufficient stimulus dogs may use inferential associations. Ament (3) reports the behavior of a two-year-old dog, who on a cold day found a window from which he was accustomed to look so coated with frost that his view was obstructed. Thereupon the dog licked away an area about the size of a plate, and then stopped, looking through the cleaned glass and observing the cats in a neighboring yard. The author concludes that here we have a case of genuine '*Ueberlegung*.' It is hardly necessary to observe that since this reported case was merely the matter of casual observation and since the situation was in no way put under experimental control, it deserves scarcely more notice than that of hundreds of cases of fabulous powers of animals reported by uncritical observers. One would indeed be rash to assert that dogs possess any degree of rational power without better evidence than that offered by Ament, although his paper appears in a journal of high scientific standing.

Opposed to such conclusions as these, but with no more scientific basis for the assertion of an opinion, is Hill, who denies any power of symbolic thought in dogs. More careful investigators and observers of animal intelligence like Morgan (37) and Mills (36), while unwilling to accept extravagant views of the mental powers of brutes, still are not quite as conservative in their opinions as is Thorndike.

Recently Cole (10), reporting an investigation concerning the intelligence of raccoons, says:

" In concluding the description of experiments in discriminating cards of different colors and intensities, I pointed out that successful reactions demand that the raccoon compare a color which has just *disappeared* with one now present. Either this or else the animal must keep track of the number of times the colors are shown, going up every other time when the colors appear alterately, every third and fourth time when they appear by twos, etc. This second explanation violates the law of parsimony, of course, and was eliminated by the fact that I secured series of perfect reactions when the colors were shown at random. When I changed for the first time from alternate showing to twos, or from twos to threes, there resulted confusion and errors quite sufficient, I think, to show that the animals distinguish one movement from two or two from three (a species of counting). As this was of no avail when the colors were shown at random, I also believe that the animal must have retained some sort of image or visual impression of the absent color and reacted to it."

It seems reasonable from the results of the present experiment to hold a middle ground and conclude that the dogs and the other animals tested did succeed in arriving at the notion that a certain color, merely as color and apart from any particular setting functioned practically for food. Of course, it is not necessary to assume that such a notion was ever raised into clear consciousness, nor that the animals had a definite recognition of it. Still they did show ability to apply their previous experience to new conditions. This, however. they did not do, it is to be observed, on their own initiative, but only when the color had been abstracted for them, so to speak. When the test of Experiment III was first presented to dog No. 2, for example, he showed no ability to react successfully to it; but after over two weeks of trial, he learned to divorce the color from its particular situation and apparently to understand that it stood in general for food. His reactions were then in nearly every trial correct, until, during the fifth period, the conditions were increased in complexity and a confusion at first resulted which caused a temporary falling of the curve, which, however, later recovered.

The significance of these results in relation to the rational powers of animals of the degree of intelligence of those tested in the experiment is that apparently they possess no initiative in the direction of abstraction, but on the other hand that they can be taught a rudimentary abstraction if conditions are artificially arranged by the experimenter; and so under very special conditions they may show an ability to develop 'free ideas' of a low grade, which ability appears, however, to be lacking under the more ordinary conditions in which the animal is placed.

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SOME PRELIMINARY OBSERVATIONS ON THE DEVELOPMENT OF INSTINCTS AND HABITS IN YOUNG BIRDS.[1]

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A. GENERAL AIM OF STUDY.

The general purpose of this study was to get some observations on the origin of instincts and on the formation of the first associations. All vertebrates, as well as several lower forms, that have been put to the test have been found capable of adjusting their behavior on the basis of some sort of memory of past experience. When we come to make observations on the natural activities of adult animals we are confronted, therefore, with a difficult problem of analysis. This is the problem of the exact relation of instinct and intelligence in any given case. Even such a familiar and much-observed phenomenon as fear in animals is not yet entirely understood as regards the nature of its origin and the type of behavior to which it belongs. The general question involved here cannot be decided by determining the degree of learning capacity, nor by establishing the fact that in certain cases under controlled conditions certain activities are acquired through experience and not inherited through instincts. Where the possibility of both modes of acquisition is given the actual fact as to the relation of these two factors in the various natural activities of animals must be determined by observation on these activities themselves. And the observation must begin before the activities in question are already formed. This takes us to the study of animal infancy, wherever a period of immaturity and development precedes the adult form, for it is during this period that many of these activities begin.

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[1] Two years ago Professor E. C. Sanford suggested to Dr. G. Ordahl that animal infancy might be a promising field of investigation. On further consultation I suggested that birds would offer the best means of approaching this subject. This study of young birds we took up together in the season of 1907, during which Dr. Ordahl did the major part of the observing. Circumstances not permitting him to continue the work, I carried it further during the following season. During the first season Robins, Chipping Sparrows and Rose-breasted Grosbeaks were observed. For the second season the place of observation shifted from Massachusetts to central Nebraska, and the birds studied were the Redwinged Blackbird, the Brown Thrush and the Mourning Dove.

The problem, however, is not so easy of solution as might seem from this statement. This study was begun with the more or less accepted view that instincts, although they may be deferred, never as such show any definite period of development; that each appears more or less abruptly in its final form without any significant change in its character, except as affected by experience. This makes gradual acquisition one of the criteria of the intelligent character of any behavior in question. Our general results, however, show, we believe, that such a view is quite untenable. A given piece of behavior may be gradually acquired in a manner and under conditions that preclude past experience playing a part; an instinct as such may show stages of gradual development. It is obvious that the proof of this suggestion may be difficult to establish. For it is often difficult to make observations so minute and complete as to justify our assuming that nothing of importance in regard to the experience of the animal in question has escaped us. And it is even more difficult to always make certain that we are rightly interpreting the significance of past experience that we have observed. Wherever such questions as these arise greater thoroughness of observation and control of factors involved alone can decide. It was the original aim to study the origin of instincts and of intelligent behavior in a number of different species in order to get observations on the greater variety of behavior that a number of different species will always show. The results of two seasons with observations on six species have, we believe, justified our efforts on this *general* problem. But certain aspects of the study have inclined me to the view that a more intensive study of one or two species only will in the end prove more profitable. This will at least give us a better and more correct comprehension of the life of the animals studied, in relation to which life alone the meaning of the particular instincts and other types of behavior must be judged. I therefore make the present preliminary report.

B. Method of Study.

The study of the natural activities of animals in their natural environment has always to contend with two difficulties. The first is that of keeping the animals under continuous and prolonged observation; they get out of range of observation, or out of sight in the woods, marshes, or in their burrows. The second is the animals' fear of man. To avoid disturbance the necessary distance between observer and observed is usually too great for much observation.

In the case of the study of young birds both these difficulties have been largely met, at least for altricial species. Since the young of these birds spend nearly the whole period of their development in the nest, they are always accessible to observation if the nest itself is. Herrick's method of getting the nest within close range of observation consisted in removing it together with its immediate supports and fixing it again three to four feet above ground.[2] This, he found, could be done after the young were several days old, without causing the parent birds to desert. He then pitched a small observation tent within a few feet of the nest, and found that usually in the course of an hour or so the parent birds resumed the care and feeding of the young in approximately a normal way. In thus eliminating the fear of the parent birds as a disturbing factor the following general facts are made use of. First, the strong instinctive attachment to the young, which increases with the age of the young. Secondly, the parent birds' capacity for learning. The stationary, silent, unchanging, and harmless tent soon ceases to arouse fear.

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[2] *Home Life of Wild Birds,* New York, 1905.

Our methods of observation were essentially the same as those of Herrick. But since our aim required observation of the young from the time of hatching it is obvious that some variations were necessary. In the first place, we never removed the nest and fixed it again close to the ground. Instead of this a mirror was placed about a foot back of and a little above the nest, hinged so as to reflect the image of the young at any desired angle. The mirror was found usually to cause little or no disturbance. An observation tent was then pitched some distance from the nest, and a field glass used to see the young in the mirror. When necessary, both mirror and tent were at first somewhat removed from the nest and gradually moved closer from day to day, beginning this before the young were hatched. The same was done with a cloth used later to cover a camera, when pictures of the different aspects of the behavior at the nest were desired. The degree of precaution necessary, as Herrick observes, depends on three variable factors: the species, the individual bird and the age of the young.

With these variations from Herrick's procedure, we have been able to keep the young of six different species under continuous observation from the time of hatching until they left the nest, extending the observations for several days after they had left the nest in a few cases. In all, thirty-two nests have been observed, but on only about twenty have the observations been systematic and continuous. In only a few cases were the observations on a nest made continuous throughout the day. In place of this a given nest was observed from one to four hours daily. As already stated, the species studied were the Chipping Sparrow (*Spizella socialis*), the Rose-breasted Grosbeak (*Zamelodia ludoviciana*) and the Robin (*Merula migratoria*) during the first season, and the Redwinged Blackbird

(*Agelaius phœniceus*), the Brown Thrush (*Harporhynchus rufus*) and the Mourning Dove (*Zenaidura macroura*) during the second. The main results will be considered under three headings: (1) Motor development, (2) the food reaction, (3) the development of fear.

C. Motor Development.

I shall use the term 'motor development' to include both growth in weight and strength, and acquisition of motor coördinations. Not considering the factors of health and nutrition so far as these are determined by external environment, the former belongs rather strictly to the realm of heredity. But the relation of heredity and exercise in the acquisition of motor coördinations has in most cases been a debated question. To my mind the question is not one that lends itself at once to a *general* consideration. It must be studied independently for each species. For obviously there is a wide range of relationship between the two factors. So far as our present knowledge goes, the motor coördinations of a large number of species do not improve from the day of birth, while among the vertebrates there are many that show a long period of functional immaturity during which motor coördinations are gradually acquired. We cannot on the one hand say, therefore, that even the highest degree of complexity in motor coördinations without exercise is impossible. On the other hand, we cannot for this reason infer at once as to the unimportance of exercise where the coördinations are gradually acquired.

Aside from this question is the equally fundamental one which concerns the relation of growth and the acquisition of coördinations. Here, too, the range of possibilities from species to species seems to be nearly unlimited. A high degree of immaturity as regards size and weight is in some species correlated with complete or nearly complete motor coördinations, while in others maturity in growth and in function appear nearly simultaneously. Like the former, therefore, this question is also not one for general consideration before the facts for the different species are known better. The facts in regard to both questions have their immediate interest in connection with the individual species. The relation existing between hereditary acquisition and acquisition through exercise, between growth and coördination, for a given species must have some meaning for that species, must in the rough at least be an adaptation for its life and life conditions. It is from this standpoint that we should endeavor first to understand the facts.

In our study an effort was made to get minute observations on the development of motor coördinations as would appear in the movements and activities of the young birds from the time of hatching until they were lost to observation after having left the nest. For the three species observed during the second season the young of a number of nests were also weighed daily, or every other day, in order to determine the nature of their growth curves. I shall give the results on the growth curves first.

1. Growth Curves.—For the nests for which the young were to be weighed observations were begun before the young hatched, to the extent of making at least daily visits. Under these circumstances the age of the young at the first weighing might vary from less than an hour to nearly twenty-four hours. But a bird nearly twenty-four hours old can be recognized as such, and in these cases the first weighing was counted as that of the second day. Birds of the same nest that hatched on different days were marked with different colors to prevent confusion later. The number of birds weighed for the Brown Thrush and the Mourning Dove is not very large, but the results are so regular and agree so completely for the three species that I offer them as trustworthy on the general characteristics that will be considered. No doubt results based on a very much larger number of cases would show minor facts that the present curves do not indicate. The following table gives the average weight in grams from the day of hatching till the birds left the nest. The day on which a bird hatched is in each case counted as the first day. This manner of designating the age will be adhered to throughout. The 'o' day then gives the weight of the egg.

Fig. A, Plate I., gives the curves based on the figures in this table, the lowermost being that for the Redwing, and the upper-

PLATE I.

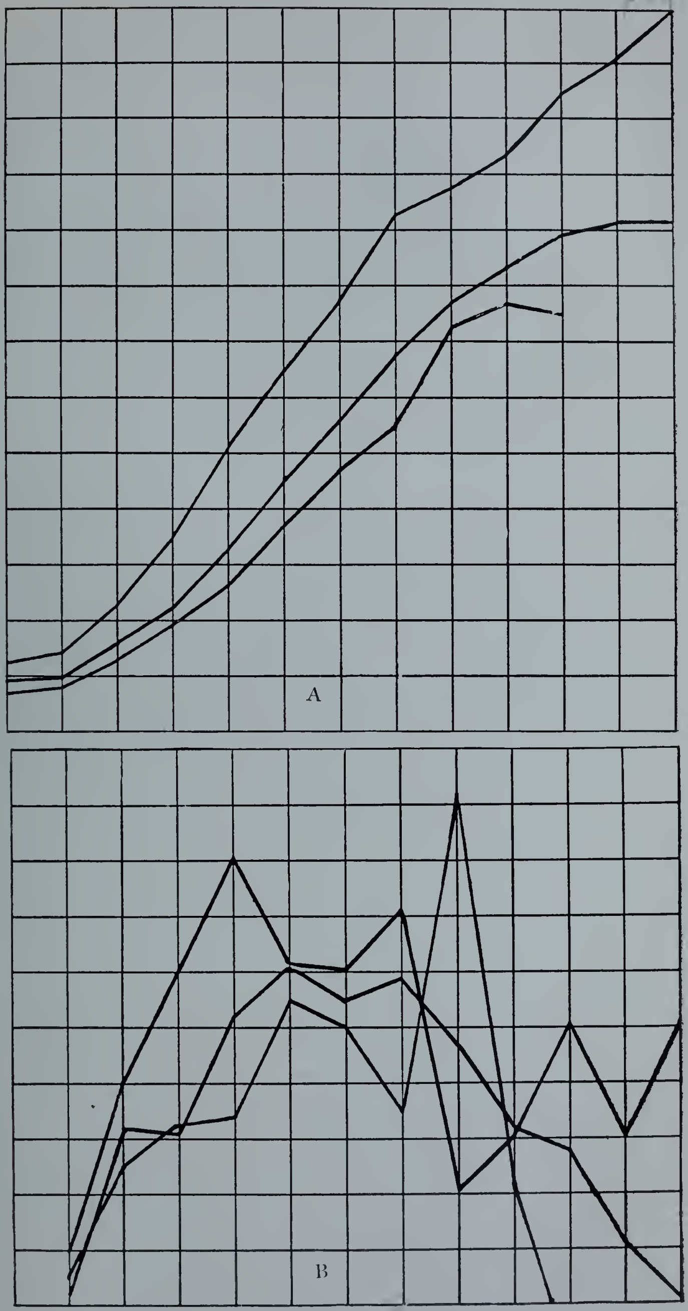

TABLE I.

Days	0	1	2	3	4	5	6	7	8	9	10	11	12
Redwing...	3.5	4.0	6.5	9.7	13.1	18.6	23.6	27.1	36.3	38.5	37.4		
No. Cases..	5	19	23	21	16	17	9	9	6	8	6		
B. Thrush..	4.6	4.8	8.0	11.1	16.3	22.4	27.9	33.8	38.5	41.7	44.5	45.6	45.7
No. Cases..	2	3	3	4	4	6	4	7	7	7	6	4	3
M. Dove...	6.1	7.1	11.4	17.5	25.7	32.5	38.6	46.2	48.6	51.8	57.2	60.3	65.6
No. Cases..	3	3	4	5	5	5	3	4	4	4	4	4	3

most one that for the Mourning Dove. They show a remarkable degree of regularity, considering the small number of cases. The young of the Redwing were not as healthy as were the others. In a number of cases they stopped growing suddenly, sometimes losing weight, and in a few cases died a day or two later. Cases that stopped growing entirely for a day or more were not counted after this cessation of growth. But the irregularity in the growth curve for the Redwing is probably due to a smaller variation in health than was considered. The Brown Thrushes seemed in every respect to be exceptionally healthy birds. It is seen that their curve is absolutely regular throughout. The curves for these three species agree in three general characteristics. There is in all a slow rise at the beginning. This is followed by a more rapid rise for several days. Thirdly, the last several days in the nest show a decline in the rate of growth.

These characteristics can be seen better in the derived curves, showing directly the rate of growth and changes in the rate. Fig. B, Plate I., gives these derived curves. They are based on the difference in weight between each day and the next. That is, the height of the curve for any day represents the number of grams increase in weight over that of the preceding day. These derived curves show clearly three periods of growth. (*a*) A period of slow but increasing rate of growth, lasting from the first to about the fourth day. (*b*) A period of maximum but constant rate of growth, lasting from about the fourth to the seventh day. (*c*) A period of decreasing rate, beginning about the seventh day.

2. THE ACQUISITION OF MOTOR COÖRDINATIONS.—For the present I shall not distinguish between motor coördination in the strict sense and neuro-muscular strength, but shall regard

the increasing ability to move the limbs and other parts in a coördinated way as a process of acquiring motor coördinations. To trace out the course of this development different nests of the six species mentioned were kept under daily observation under conditions that seemed, so far as we could determine, entirely normal for the birds in question. In this the camera was an important supplement to the note-book, in as much as it furnished illustrations of the normal attitudes of the young at different ages, which could be brought together and compared later in a way that description alone does not make possible. In the following description the ages given for the different periods of development refer mostly to the Redwing. The course of development, however, is exactly the same for the other species, excepting the Mourning Dove in some minor matters. The species observed vary simply as regards the rate of development.

(*a*) *First Stage.*—At the time of hatching the young are nearly inert. The legs and wings project loosely from the body, and during the first day the head and neck are coiled ventrally most of the time. The legs are entirely useless, and play no part in the movements of the body, which is a shapeless mass of pot-belly and head. The movements are mostly incoördinated wrigglings, which are not always sufficient to right the bird if by accident it has been turned onto its side or back. In this event it simply continues its wrigglings until by accident it rolls back again into the normal position. Plate II., 27.1, shows a bird on the second day after hatching, lying on the side and struggling to right itself. (The numbers in this plate and in the following ones give the age of the bird, and the nest and negative numbers. The number under each individual bird gives the age, counting the day of hatching as the first day. In the other the number preceding the decimal point is the nest number, that following the decimal point is the negative number for that nest. The former will serve to indicate whether successive illustrations are of the same individual birds or from different nests.) The only coördinated movement present is the food reaction. This consists of raising the head and neck into a vertical position and a wide gaping of the mouth (Plate

III.). The stubby wings usually assist some from the outset in attempting to brace and raise the anterior part of the body at the same time. This use of the wings continues and increases during the next several days, but without the energy necessary to raise the body very much (Plate III.). By the second or third day the neck and head have become quite uncoiled from their ventral position. The birds now normally lie stretched out flatly, the head and neck lying loosely and limp as did the legs and wings from the first (Plate II., 24.4, Plate III., 18.1). The shape of the body has changed slightly, presenting a wider and flatter ventral surface. They have less difficulty in righting themselves when turned over. The pliant viscera of the pot-belly is beginning to serve as ballast and a foot for keeping the bird in a normal position, which function it fulfills for the next five to six days. By the fifth or sixth day the first stage of acquisition of motor coördinations is completed. The legs are now usually drawn in and folded under the body instead of extending loosely. The head is also pulled in, shortening the previous slender neck in appearance. The wings hang about as before (Plate II., 21.3, 19.2, 6, 7). The food reaction is given with much more vigor, and the wings are used more than at first to brace and raise the body. There is a marked increase in neuro-muscular strength. When arranged in the nest with the heads away from the center, which is a position the young begin to take by this time, the pushing of the wings against the sides of the nest often enables them to raise themselves upon end. In this the legs play no part, but come up in a vertical position with the body, remaining folded against it as in the reclining position. When the birds are taken out of the nest and placed on the smooth surface of a board the legs, if used at all, make only random sprawling movements that do not raise or move the body. In their normal position at rest they are folded in under the body as when the birds are in the nest.

(*b*) *Second Stage.*—From the fifth to the seventh day inclusive there is little or no progress in the motor coördinations. We may call this period, therefore, the second and stationary stage of development. The birds become more active and vigorous, but otherwise the nature of their movements does not change noticeably.

(*c*) *Third Stage.*—By the seventh to the eighth day this stationary period is broken. From now on rapid progress in motor coördinations is made. I shall call it the third stage, which ends with maturity. The wings now begin to be folded more closely to the body. The bird begins to use the legs to raise the body (Plate II., 19.2, 8, 8). With this the wings cease to be used for this purpose in the food reaction. For several days the first joint of the legs, from the foot to the knee, is still used as foot, and the foot alone is not used to stand on as in the adult bird before the nest is left (Plate II., 21.8). By the eighth day the birds use the toes to grasp a stick and will remain perched on one if carefully placed there, although their bodies lie down flat upon it at the same time. By the tenth day they can maintain a standing posture, and will sit somewhat erect when perched on a stick. During this period such activities as scratching the head, and preening the feathers appear. The Grosbeak, Chipping Sparrow and Redwing usually leave the nest between the ninth to twelfth day. The Brown Thrush, Robin and Mourning Dove leave it two to five days later. At this time they can fly from a few feet to a hundred yards, depending on whether they leave the nest a few days early or late. The second and third attempt, if made immediately after the first, is much less successful. A young bird that can fly one to two hundred yards the first time can usually be caught the third to fifth time, if followed up immediately, in all probability simply a matter of fatigue. Their manner of flying at this time is readily recognized as that of a young bird. It is slow, and the body is carried more in a diagonal instead of a horizontal position, as is the case with the adult bird. The stroke of the wings also seems somewhat different, but in what ways has not been determined.

(*d*) *Relation of Growth in Strength and Acquisition of Coördinations Proper.*—In these three stages of acquisition of motor coördinations we must now distinguish more sharply between growth in neuro-muscular strength and acquisition of coördinations as such. The observations on the relation between these two factors may be expressed briefly in the generalization that *coördination*, the ability to use the right muscles in the right combinations and order, *appears and develops before the neuro-muscular strength necessary to make the movements effectively.* This is seen best in the use of the wings to brace and raise the body in the food reaction, and in the efforts to stand, to raise the body on the feet. The wings are used in this way from the time of hatching till the beginning of the third stage of motor development, when a standing posture begins to appear. The manner of use does not change essentially during this time, but its efficiency due to growth in strength does. This is seen quite well in the illustrations in Plates II. and III. In like manner efforts to raise the body on the feet begin in a coördinated way before there is sufficient energy to do so. From the seventh to ninth day the Redwing was often observed to crawl backwards when placed on a smooth surface. On a closer study of this behavior it was found to be due to this attempt to stand when sufficient strength was still lacking. The body is raised only slightly and the direction of the force from the contracting leg muscles causes the body to fall backwards, leaving the feet in a forward position. Further attempts to stand then pushes the body backwards directly. This position is well illustrated in Plate II., 21.5. A study of Plate II., 21.3, 7, 19.2, 21.5 and 21.8 will show the mechanism of this behavior, and also the development of neuro-muscular strength following that of coördination. It might be added also that the wings are used in a coördinated way for flying before there is strength enough to fly. But here the matter is complicated by the fact that the wing feathers are not full-grown until some time after the coördinations appear. The prominence of fatigue, however, in the first repeated efforts at flight just after the birds leave the nest is again an illustration of this phenomenon.

(*e*) *Relation of Heredity and Exercise.*—The observations on the acquisition of motor coördinations and neuro-muscular strength may justify a word in regard to its hereditary character, and the rôle of exercise. They have left the general impression that exercise plays little or no part in this acquisition.

But a proof of any such assumption is at present quite impossible. The impression gains its force from three sources. First, from the fact just considered, namely, that coördinations precede efficient neuro-muscular strength. If exercise were required to establish these coördinations one should expect that on the whole the order would be the reverse. There should be first the required strength to make movements, which with repetition would fall into the right coördinations. Secondly, some of these coördinations are present from the start. The complex coördinations of the muscles of the head, neck and wings in the food reaction at the time of hatching are purely hereditary. Thirdly, the amount of exercise for the different activities observed is in some cases not very great. Trying on the wings for flying does not occur until a few days before the birds actually fly. And during these days it may not occur more than several times, as was found when a nest was kept under constant observation throughout the day. Such activities as scratching the head while standing on one foot, and preening the feathers were, when observed for the first time in a nest, not different in character from what they are later, except in ways that can be best explained on the basis of lack of strength. Yet it is evident that we cannot conclude from present observations that exercise played no part at all in those aspects of the different coördinations that were acquired gradually.

3. Relation of Growth in Weight and Acquisition of Coördinations.—We may now consider briefly the relation these observations show between growth in weight and the acquisition of coördinations, using the latter term now again to include both coördinations proper and neuro-muscular strength. The growth curves showed very distinctly three stages of growth that are all quite definitely limited as regards the ages at which each begins and ends. We have also described three stages in the acquisition of motor coördinations. A comparison of the results in the two cases will show that the stages in the latter are fairly closely correlated with the three stages of growth in weight. The first stage of slow but rapidly increasing rate of growth is correlated with acquisition in motor coördinations. The second stage of maximum but

constant rate of growth is correlated with no noticeable progress in coördinations. The third stage of decreasing rate of growth is correlated again with progress in acquiring coördinations. In more general terms, progress in coördinations is greatest when growth is slowest, and while the rate of growth is at its maximum acquisition of coördinations ceases. I am not prepared at present to discuss very much the significance of this correlation. However, it is not to be inferred at once that there is any necessary connection between the two. An improvement in the processes of digestion and assimilation might be responsible for the initial slow rate of growth, while the final decrease in the rate is undoubtedly due here to the same physiological changes that give rise to the same thing in all animals. On the other hand, the rapid acquisition of a few coördinations during the first few days and the delay for the rest until the last few days spent in the nest may be the result of an hereditary adaptation. The coördinations are acquired as they are needed. And it is just as essential that the latter, the ability to use the legs in standing and walking and the ability to fly, should be delayed as that the other coördinations should be developed early. It is their motor immaturity alone as regards these coördinations that keeps the young in the nest and prevents their destruction. These things suggest the possibility that the correlation in question may be incidental, that the stages in growth and in acquisition of motor coördinations are independent. But we shall return to this later.

D. The Food Reaction.

From the nature of the case, the first food reactions must be purely instinctive, both with reference to the nature of the motor response and the kind of stimuli that arouse it. But in regard to the latter at least there is from the first abundant occasion for associations between different stimuli and food to become established. The young are fed at more or less regular intervals throughout the day, varying from about once every five minutes to once every forty minutes. The parent birds have different calls, some of which they use exclusively for the purpose of arousing the food reaction, and others for other occasions. As they light on the nest they jar it, varying in degree with the manner of construction and support of the nest. After the young are several days old their eyes open, giving the possibility of associating food with the sight of the parent birds. There is then a variety of stimuli with which the young may come to associate food. For this reason the food reaction gives an excellent opportunity for studying the relation of instinctive and intelligent behavior in its reciprocal development. I shall consider (1) the nature of the motor response in the food reaction, and (2) the stimuli that arouse it. In this the Mourning Dove will not be taken into account, since it feeds its young in a different manner, only a few times a day with large quantities of regurgitated food at a time.

1. NATURE OF THE MOTOR RESPONSE.—The motor response consists at first of raising the head and neck into approximately a vertical position and a very wide gaping of the mouth. With growth in strength and coördinations in general, more of the other muscles come into play. But the essential character of the motor response does not change until several days after the young have left the nest. That is, it is not until this time that the young begin to use their bills in pecking at things and to actively take hold of food that is given them. Throughout the period of growth and later the parent bird puts the food well down the throat of the young, leaving nothing to the latter but the reflex act of swallowing. It is then only in the unessential movements involved in raising the body and in the use of the wings that the total motor response in the food reaction develops during the period of growth (Plate III.).

The food reaction is given repeatedly as long as the stimulus for it remains, and continues for some time after the bird has just been fed. The vigor and promptness with which it is given varies with age. It is at its maximum in this respect during the period of maximum rate of growth. But this is most likely due to the young being hungrier at this time. Since they grow faster, they need more food, but the observations so far do not indicate that they are fed more abundantly during this period. The character of the response in this respect naturally varies at any age with the degree of hunger, and undoubtedly with other physiological states. At any age it also varies with different stimuli, all of which at the given age still call it forth in some degree. When well fed no stimulus may call forth a very vigorous response, while when specially hungry a stimulus that usually gives no response may arouse a very vigorous reaction. These variations in the nature of the response, and as to whether it will be given at all or not, as dependent on these several conditions, are of the first importance in considering the different stimuli that call forth the food reaction at different ages. It results in an irregular course of development that may show only a general progress in a given direction. On any given day in the particular case the reactions may be what they normally are a day or two earlier or later. We shall see that this is in general the case.

2. STIMULI THAT AROUSE IT.—Two methods were employed to determine what the stimuli were to which the food reactions were given at different ages, for it was found at once that they changed with the age of the young. The first was simply that of general observation, noting as carefully as possible the natural conditions under which the young reacted. The second consisted of testing their food reactions daily with artificial stimuli, some of them being rough imitations of the natural stimuli. The latter was carried out systematically only with the Redwing and Brown Thrush. The artificial stimuli thus employed were as follows: (1) Imitated cluck of the parent bird, (2) a one-note whistle, (3) a clap of the hands, (4) a short hiss, (5) jarring of nest by tapping it with a pencil, (6) a rapid pass with the hand and extended first and second fingers towards the young. Each was repeated once or twice at about one second intervals. An obvious objection to testing the food reaction with these artificial stimuli is that the manner of their production is very crude and poorly standardized. We shall see, however, that in spite of this a number of things have been determined by their use.

(*a*) *The Natural Stimuli.*—The observations on the natural conditions and stimuli that arouse the food reaction will serve to give a general account of the changes that occur with age. I shall give these first. During the first day or two many of the food reactions are of a spontaneous character. When there are no observable external stimuli of any sort present and all the young are lying perfectly quietly in the nest, one will occasionally give a food reaction, the head dropping down again after two to three seconds. These spontaneous reactions disappear very soon. Usually none occur after the first day. The first external stimuli that arouse the food reactions are the special clucks of the parent birds, used especially for this purpose, and tactual stimuli through jarring of the nest when the parent bird lights on it, or from the wrigglings of the young themselves in the nest. According to the observations, the cluck from the first arouses the reaction a little more readily than do the tactual stimuli. But since the observations usually began when the young were already several hours old, we cannot be certain that the effect of experience was not already present. Experience would effect the establishing of the association between food and the cluck, and between food and the tactual stimuli differently. For the young may move about in the nest more or less constantly, and reactions to this are then not usually followed by food. The same holds true for the jarring of the nest when the nature of its supports allows it to swing and shake in the wind. This might tend at first to eliminate the reaction to tactual stimuli until the particular kind of jarring due to the old bird lighting on the nest is discriminated from the rest. In the reactions to the different artificial stimuli we shall see the evidence for this. Individual birds of the same species vary widely in the amount of use they make of the cluck to arouse the food reaction. But in most cases they have been observed to resort to this when the food reaction was not given immediately on their arrival at the nest. After a few days the predominant mode of reaction is to the jarring of the nest caused by the parent bird lighting on it, and is given promptly to this stimulus. The cluck is eliminated because it is not required, and the young do not react to other calls of the parent nor to the sound of its wings when it flies within a foot of the nest without lighting on it. Individual habits of the parent birds and the structure of the nest may cause exceptions to this rule. By the second to third day after the eyes of the young open they begin to give the food reaction to the sight of the approaching parent. In the course of another day or two this becomes the predominant mode of reaction.

On the whole, then, we have several definite changes in the stimuli that arouse the food reaction. With any individual nest these changes are often quite abrupt, being made completely in the course of twenty-four hours. These changes seem to be made entirely on the basis of experience. The young simply learn progressively to associate food first with the cluck, then with the jarring of the nest, then with the sight of the approaching parent bird. As fast as this takes place the stimuli that aroused it at first are eliminated as a means of arousing it, first the cluck because it is no longer given when it is no longer needed, then the jarring of the nest because the reaction takes place through sight before the nest is touched. That food should become associated first with the cluck rather than with the jarring of the nest as the parent bird lights on it, is accounted for by the fact already stated; the tactual stimulus from this jarring has to be discriminated from other tactual stimuli, from that of the wrigglings of the other young in the nest, from that of the parent bird brooding them, and from that of the swaying of the nest in the wind. All that is required is that the reaction should at first take place instinctively to any kind of stimulus, no matter what its nature. Experience can then perhaps teach the young to inhibit the response to some stimuli as well as to give it more readily to others. The results obtained with the use of the artificial stimuli with the Redwing and the Brown Thrush will throw more light on how readily these associations are formed, but will also show that the reactions in this case may not be so simply explained.

(*b*) *The Artificial Stimuli.*—In preliminary tests with a large variety of artificial stimuli it was found that just after hatching the young would give the food reaction to practically any kind of auditory or tactual stimulus. But later they ceased to give this reaction to most or all artificial stimuli.

We seemed to have, then, in this food reaction an illustration of a law observed in other aspects of animal behavior, namely, a definite instinctive reaction aroused by no one definite stimulus only, but by a large range of quite different stimuli, which through experience comes later to be aroused by certain stimuli only. To test this idea the artificial stimuli used to study food reactions were so chosen as to approach the character of the natural stimuli in different degrees. The imitated cluck, the whistle, the jarring of the nest all approach the character of the latter in some measure. The clap of the hands and the hiss in no way, excepting duration, resemble anything the young birds experienced in their natural environment. If then experience and association alone produced the progressive changes in the food reaction as regards the stimuli that arouse it, those artificial stimuli that resemble the natural stimuli most should continue longest to do so, while those that resemble the natural stimuli least should soon cease to arouse the reaction. In general, the results show that this is the case. But there are exceptions and certain aspects of the results which strongly indicate that the matter is complicated with other factors. We may turn now to these results.

During the first two days all the stimuli used, excepting of course the visual one, aroused the food reaction about equally readily. By the third to fourth day differences for the different stimuli appeared. Food reactions to the whistle and clap of the hands practically ceased after the fourth day. To the hiss they ceased one to two days earlier. To the imitated cluck and jarring of the nest they ceased only after the seventh day. But while in the case of the former the food reaction was at its maximum during the first two days, for the imitated cluck the reaction was given most readily from the third to the fourth day, and for the jarring of the nest it was most prominent from the third to the sixth day. The food reaction to the approach of the hand and extended first and second fingers occurred for the young of some of the nests only, and could never be aroused in the majority of cases. It occurred from the eighth to the tenth day. Put into brief form and excluding scattering exceptions, we have, for the Redwing alone, the following periods during which the food reaction was given to these artificial stimuli.

Imitated cluck	first to seventh day.
Jarring of nest	first to seventh day.
Whistle	first to fourth day.
Clap of hands	first to fourth day.
Hiss	first to third day.
Sight of hand	eighth to tenth day.

These figures represent the limits within which the food reaction to each of the stimuli used began and ceased to be given by *any* of the young observed, excepting a few individual instances of reactions that were very clearly quite exceptional. Individual variation, however, and variation with the physiological conditions mentioned were large. It follows, therefore, that for any particular bird, these periods were usually quite different. The changes in the reactions were also often strikingly abrupt. It will be important to keep these facts in mind. A more correct idea of the nature of the changes for any particular bird is to be gained by considering some individual cases. The following is a typical record for the Redwing:

No. 28. Redwing.—Two eggs, first hatching June 22, the second, June 23.

June 23. First and second day. Both give the food reaction to all the stimuli.

June 25. Third and fourth day. Both give the food reaction to the imitated cluck. Both give no reaction to the whistle, clap of the hands, jarring of the nest. Both give slight reaction of fear to the hiss.

June 26. Fourth and fifth day. Both give no reaction to the imitated cluck, whistle, clap of hand, jarring of nest. Both give reaction of fear to hiss.

June 28. Sixth and seventh day. Both give no reaction to whistle, and to clap of hands. Oldest gives slight reaction of fear to imitated cluck, pronounced reaction of fear to hiss. Youngest gives slight reaction of fear to hiss.

June 29. Seventh and eighth day. Both give reaction of fear to all stimuli.

June 30. Eighth and ninth day. Both give slight reaction of fear to imitated cluck, pronounced to hiss; no reaction to other stimuli.

July 1. Ninth and tenth day. Both give reaction of fear to all stimuli.

July 2. Both have left the nest.

The following is a record for the Brown Thrush:

No. 35. Brown Thrush.—Three eggs, first hatching July 22, second and third, July 23.

July 22. First day. Food reaction is given to all stimuli excepting the clap of the hands.

July 23. First and second day. All give food reaction to the whistle and to the imitated cluck. All give no reaction to jarring of the nest, or to the hiss.

July 24. Second and third day. All give no reaction to any of the stimuli.

July 25. Third and fourth day. All give no reaction to any of the stimuli.

July 27. Fifth and sixth day. Reactions same as on July 24 and 25, excepting that the oldest gives reaction of fear to the hiss.

July 28. Sixth and seventh day. All give reaction of fear to the hiss. All give no reaction to all other stimuli.

July 29. Seventh and eighth day. All reactions the same as on July 28.

July 30. Eighth and ninth day. All give food reaction to approaching hand and extended finger. All other reactions same as on July 29.

July 31. Ninth and tenth day. All reactions same as on July 30.

August 2. Twelfth and thirteenth day. All have left nest.

These records may be taken as representing the general result on the degree of regularity of the changes and their abruptness. The latter, however, is seen more strikingly in some of the illustrations of Plate III. In 18.1 we see the two younger ones, first day, giving the food reaction to the whistle, and the two older ones, second day, failing to respond. In 21.4, two, sixth and seventh day, are giving the food reaction to the imitated cluck, and the largest one, seventh day, failing to respond. In 32.1 all are giving a very vigorous food reaction to the imitated cluck. In 32.3, a day later, the two older ones, fifth day, have ceased to react to this stimulus.

In these observations with the artificial stimuli we may now call attention to several things which make the interpretation of the changes in the food reaction to this class of stimuli on the basis of experience and the formation of associations alone difficult. The first is the fact that the food reaction ceases for the hiss before it does for the clap of the hands. Both of these would seem to be about equally unlike any of the natural stimuli, and if this is correct the reaction to them should be equally affected by experience. Secondly, we see that stimuli that at first arouse the food reaction later give a reaction of fear. Thirdly, the change in the reaction to the different stimuli, from food reaction to no reaction, and from no reaction to the reaction of fear, is often quite abrupt. In this connection we may add at present that the process of forming an association is a gradual one. In the present case it is not completely formed between food and the cluck until the third to fourth day, and for the jarring of the nest it appears at its strongest slightly later still. On the other hand, we shall see later that changes in the reaction to fear are purely instinctive and occur abruptly. From these facts it becomes obvious that the changes in the food reaction as due to the formation of associations may be complicated with a developing instinctive fear. I shall postpone the full interpretation of these observations until after we have considered the development of fear.

E. The Development of Fear.

The present-day view of fear in birds, as in other animals, which is most widely accepted is that it begins in the form of a generalized instinct. Birds first fear the unusual and the strange in general, and later come to fear particular things as a result of unpleasant experience having become associated with them. Different species show fear in various degrees, and the age at which it appears also varies with the species. For the class of birds that we have observed Herrick states that fear is usually shown about the time the wing feathers develop, that is, in general, a few days before they leave the nest, and that it appears quite abruptly. At this age young birds are apt to leave the nest through fear when approached too closely. To Herrick's observations we shall be able to add that fear in other forms is shown at a much earlier date, and that in some birds its form of manifestation changes several times from its first appearance until maturity is reached. On the basis of our observations on the food reaction and the development of fear together, I shall also attempt to justify some modification of the general view of the relation between instinct and association in the fear of the adult animal.

The method of studying the development of fear in these birds was limited to the use of the artificial stimuli. In their natural environment few things occur that occasion the arousal of fear. General observation without the introduction of other than the natural stimuli can therefore yield but few results on the nature of its latent development. The artificial stimuli employed in a systematic way for this purpose were those used at the same time for the study of the development of the food reaction. In a less systematic way some of these and others were used in observations on the Chipping Sparrows, Robin and Grosbeak. We shall consider (1) the different manifestations of fear and order of development, and (2) the nature of its development and relation to the nature of the stimulus.

1. *The Different Manifestations of Fear and Order of Development.*—With individual exceptions, fear appeared in five different forms from the time of hatching until the young left the nest. These may be stated briefly as follows: (*a*) I shall regard the cessation of giving the food reaction to stimuli that at first aroused it as due in part to a developing latent fear. The justification for this will appear later. (*b*) The first external and visible reaction of fear consists of a sudden shrinking or shiver to a stimulus. This lasts only a second, and does not involve a special use of the limbs or change in position. (*c*) Following this is a period during which the bird crouches to a stimulus. In this the limbs and head are drawn in, and the birds hug closely the bottom of the nest, usually closing the eyes if they are already open, and lying perfectly quietly for some time after the stimulus (Plate IV.). (*d*) Next is a briefer period during which the birds may not crouch, but remain on the alert and with wide-open eyes follow moving objects in an attitude which may be interpreted as that of alarm (Plate IV.). (*e*) Soon after this the birds will attempt flight and escape when approached. After dropping to the ground from inability to fly or from fatigue they will run to cover, hide and crouch close to the ground. Of these different manifestations of fear the first three were not observed in the Mourning Dove, the first necessarily not, because it gives no food reaction to any of these stimuli. The second was not noted in the Chipping Sparrow, Robin and Grosbeak, but is probably present in these also, simply having escaped observation because it was not systematic enough on this particular point. For the Robin a sixth is to be added, which we may call fighting fear. The first evidence of this appears in connection with the last part of the third stage of development, the crouching in the nest. In this crouch the robin will sometimes simultaneously open its bill in an attitude of fight (Plate IV., 7.11). Later, it will defend itself in this way when caught (Plate IV., 7.13). With

these exceptions, all the birds observed showed a progressive development of fear through the five stages or periods given. Fear in these birds is, therefore, not long delayed, and its development is in one sense quite gradual rather than abrupt. It passes through several forms of manifestations which on the whole are increasingly expressive of fear. The change from one form of manifestation to another, however, is a different matter. We may consider now the nature of these changes and their relation to the nature of the stimulus.

2. *Nature of Its Development and Relation to the Nature of the Stimulus.*—I shall consider each of these several manifestations of fear separately, and in the order given. (*a*) A period of no visible external reaction to all stimuli, excepting the hiss, that at first aroused the food reaction preceded in all cases observed the first visible manifestations of fear. This cessation of the food reaction is with the individual bird usually quite abrupt. It may react quite readily to all these stimuli on a given day and fail entirely to respond to any of them twenty-four hours later and after that (See Plate III. again). But variations in hunger and probably other physiological conditions often make this first change irregular, so that for a while on different hours or days it may alternately give the food reaction or make no response. For a group of birds considered together the ‘ no reaction ’ will occur from the first to the second or third day of the third stage of development of fear, the crouch in the nest. For such a group the no-reaction will then be quite infrequent for all stimuli during the first few days, will increase in frequency at different rates for different stimuli, and cease abruptly for all stimuli by about the eighth day. For the Brown Thrush it appeared distinctly earlier than for the Redwing. This general description holds true entirely only for the behavior to the whistle and the clap of the hands. For the Redwing no intermediate period of no reaction was observed for the hiss. It practically ceased giving the food reaction to this stimulus by the third day, and gave the reaction of fear, shrinking, to it as early as the second day. Minor differences as dependent on the nature of the stimulus were observed for the imitated cluck and for the jarring of the nest.

For the latter the no-reaction was more frequent during the first three days than it was for the other stimuli, and decreased in frequency from the third to sixth day. For the imitated cluck it was less frequent than for other stimuli until about the seventh day. These differences in the reactions for the different stimuli, for this and other stages in the development of fear, are important for the general interpretation of the results.

(*b*) The shrinking from a stimulus, consisting of a sudden contraction of all the muscles and immediate relaxation, was never aroused by any of the stimuli except the hiss. It belongs to a period extending from the second to fifth day, inclusive, during which period the other stimuli aroused either the food reaction or no reaction at all. The low degree of motor development at this time makes the crouch for the most part impossible. But since the reaction is so distinctly different in character from the crouch in other ways than would be determined by this motor immaturity, it is not to be regarded as the same reaction. The crouching in a measure hides the young in the nest, and is probably adaptative. We can see no such purpose or adaptative value for the shrinking.

(*c*) The hiss aroused the first definite crouching in the nest (Plate IV., 32.2). For all the other stimuli the crouch was observed for the first time on the sixth day, and by the seventh it was already pronounced.[3] The transition to this manifestation of fear was, therefore, very abrupt. As already noted, food reactions to all stimuli, excepting occasionally the visual one of the approaching hand, ceased the seventh day. From the seventh to the ninth day then the behavior to all stimuli was either no reaction or a crouch, the latter increasing rapidly in prominence and frequency. By the ninth day all stimuli always aroused a crouch in the case of the Redwing, the Robin and the Brown Thrush (Plate IV., 21.7, 7.11, 32.2).

(*d*) During the latter part of the period in which the birds crouch in the nest to different stimuli they show alarm to moving objects, when they are placed outside the nest. Occasionally at this time they show this alarm while in the nest instead of crouching. This is true of all when just on the verge of the next stage of development, and is the rule for the Grosbeak several days earlier. For the Redwing it appears about the tenth day, for the Brown Thrush, Mourning Dove and Robin from one to five days later. This is a short period. A day or two later this suspicious glance on being approached is followed suddenly by a wild scramble out of the nest or by flight, according to the degree of motor maturity (Plate IV., 4.2, 21.9, 38.4). This last stage or form of manifestation of fear (*e*) may then be regarded simply as a further development of the process in the preceding stage. The look of alarm is anticipatory to the attempt to escape.

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[3] A buzzing sound made with vibrating lips or tongue was for the Robins found to be equally effective with the hiss in arousing the crouch before other stimuli did. This sound was not used for the other species.

3. *Its Instinctive Character.*—After this description of the development of fear it should be unnecessary to discuss the question as to whether these different manifestations of fear are purely hereditary or are due in part to experience. I shall regard the observations as conclusive on this point. These reactions are purely instinctive in their origin, affected by experience only on the side of inhibition. Experience with a given stimulus may inhibit the reaction of fear, but there has been nothing in the experience of these birds which would cause them to fear these stimuli in the first place; there could have been no unpleasant association connected with them. Further, experience could not have taught them to express their fear in these several different ways, and to make the changes at just the periods as observed. Thirdly, some of the changes occurred very abruptly, while the formation of associations in other spheres is seen to be more gradual.

F. The Relation of Instinct and Association in the Development of their Behavior.

We are now in position to attempt an interpretation of the exact relation of the factors in the changing behavior to the different stimuli that first all arouse the food reaction and later all arouse a reaction of fear. Without reviewing the observations for this purpose, let me state my interpretation now in brief terms. The food reactions to all stimuli alike at first are purely instinctive. With the frequent repetition of some of them and feeding following immediately, the association between these and food becomes established more or less gradually. This involves at the same time a growing discrimination between different stimuli, which immediately after hatching is probably almost entirely absent, and which remains very crude through the whole period of growth. This growing power of discrimination is seen in the difference in the nature of the changes in the reaction for the different artificial stimuli, which resemble the natural ones in different degrees. That it is very crude during the larger part of the period of growth is seen in the fact that the discrimination between the artificial and the natural stimuli is absent so long where they resemble each other so little. The strongest evidence for this is in their inability to distinguish an approaching hand from the parent bird when they are already eight to ten days old. This discrimination between stimuli that mean food and stimuli that do not, between the imitated cluck and the hiss, for example, might perhaps alone account for the food reaction coming to be given more readily for the one class and to be inhibited for the other. But if the observations on fear are also taken into account in this connection there is some indication that this instinctive fear is a factor in the inhibition of the food reaction to certain stimuli that at first aroused it. In other words, some degree of association of food with certain stimuli follows at once the ability to discriminate the stimuli, and with this discrimination and association goes simultaneously an instinctive fear of all stimuli with which food is not associated. From this point on, ability to make discriminations, formation and strengthening of associations, and intensity of instinctive fear develop together, probably through reciprocal influences, with only a certain degree of independent development. Special evidence for the instinctive character of the inhibition of the food reaction is given in the abruptness of its appearance in individual cases, and in the abruptness of the changes from one manifestation of fear to the next form.

In connection with this interpretation, and the general characteristics of fear as observed, we may suggest some modification of the general view of the relation between association and instinct in fear in animals that is at present most widely accepted. This general view, as stated above, is that fear begins in a generalized instinct causing animals at first to fear everything that is unusual or strange. Later they fear particular things only, and because unpleasant experiences have become associated with them. Our observations verify the first part of this view. But the early appearance of fear, and its intensity purely as an instinct do not suggest that conditions will arise very readily under which the animal can learn through experience whether certain things are to be feared or not. An animal will fear and continue to fear instinctively everything for which it has no great use, or for which there are no special causes for not avoiding it. The particular things feared later, then, are those that have not yet been eliminated through the taming process through special conditions arising that have overcome the original general instinctive fear and thus made experience with these things possible. This makes it more likely that the fear for particular things only is still instinctive than that it is acquired through experience. Indeed, the process suggested here would seem to have the greater biological value. If the original instinctive fear were overcome excepting under conditions that put a premium on boldness many animals would be destroyed before learning much through experience.

G. The Relation of Growth to the Development of Associations and Fear.

We have seen above that there were three periods of growth; a period of slow but rapidly increasing rate of growth from the first to the fourth day, a period of maximum and constant rate of growth from the fourth to the seventh, and a period of decreasing rate after the seventh day. I have also described three periods in the acquisition of motor coördinations and pointed out that these were fairly closely correlated with the three periods of growth. A few coördinations are acquired during the first period of growth, with practically no progress in coördinations during the second period, and a rapid progress with the beginning of the third period of growth. We now see that the first period of growth is also the time during which the first crude discriminations and associations are established. After the fourth day the food reaction ceases rather abruptly for the whistle, the clap of the hands, and has already ceased a day or two earlier for the hiss. These are the artificial stimuli that are most unlike the natural stimuli. During the next period of growth from the fourth to the seventh day the readiness and frequency with which the food reactions are given to the imitated cluck and to the jarring of the nest increases some. This is the effect of a growing association between food and the natural stimuli, which these artificial stimuli resemble most, combined probably also with slow progress in the ability to make the finer discriminations between these imitations and the natural stimuli. With the beginning of the last period of growth comes an abrupt change in nearly all the reactions. The food reaction is no longer given to any of the stimuli, excepting occasionally the visual one, and manifestations of fear appear definitely and quite abruptly in the form of crouching in the nest, which now very rapidly becomes the predominant reaction to all artificial stimuli, followed in rapid succession by other forms of manifestation of fear for these stimuli. This correlation between growth and progress in the development of associations and of instinctive fear for these stimuli strengthens the probability that the correlation between growth and acquisition of motor coördinations is due to some inherent connection and not incidental, as was suggested before. These correlations as shown by these preliminary observations raise a host of highly important questions on which the present results need further investigation before a definite decision can be offered.

H. SUMMARY.

The relation of instinct and intelligent behavior in the different natural activities of adult animals is a fundamental problem. Its solution demands the study of animal infancy, during which period many of these activities are acquired. There is great need of more efficient methods of getting systematic observations on these natural activities.

For the species observed, there are three distinct periods of growth in weight. First, a period of slow and increasing rate of growth, from the first to about the fourth day, inclusive. Second, a maximum and constant rate, from about the fourth to the seventh day. Third, a decreasing rate of growth, beginning about the seventh day.

There are three stages in the acquisition of motor coördinations, fairly closely correlated with the three periods of growth. During the first a few coördinations are acquired. The second stage, from about the fifth to the seventh day, is characterized by a lack of progress in acquiring coördinations. The third, beginning about the seventh day, is characterized by a rapid development of coördinations.

Coördinations proper, the ability to use the right muscles in the right order, precedes the neuro-muscular strength necessary to make the movements effectively.

Exercise probably plays little or no part in the acquisition of motor coördinations.

Just after hatching, the food reaction is a purely instinctive response. For the first day or two it is often given in the absence of any observable external stimulus. For the first several days it is also given instinctively to practically every kind of tactual or auditory stimulus. A definite association is formed first between the special cluck of the parent bird and food. Next this association is formed with the tactual stimulus from the jarring of the nest as the parent bird lights on it. The reason for this order is probably to be found in the fact that the young have to learn first to discriminate between this particular tactual stimulus and several others resulting from movements of the other young in the nest, from shaking of the nest in the wind, and from the parent bird brooding them. By the second or third day after their eyes open the association is formed between food and the sight of the approaching parent bird. These changes in the food reaction from its initial instinctive form are probably all to be explained on the basis of experience and association. But changes with age in the reactions to certain artificial stimuli (imitated cluck, a one-note whistle, a clap of the hands, a jarring of the nest by tapping it with a pencil, a short hiss, and rapid approach of the hand and extended first and second fingers) cannot be entirely explained in this way. Just after hatching the food reaction was given to all artificial stimuli about equally readily. It ceased to be given to the imitated cluck, and to the jarring of the nest by the seventh day, to the whistle, and to the clap of the hands by the fourth, to the hiss by the third, and to the sight of the approaching hand it was occasionally given from the eighth to the tenth day. For individual birds these periods were usually quite different, and the changes were often very abrupt. The interpretation of the changes in the food reaction to these artificial stimuli must take into account the simultaneous changes in the developing reactions of fear, given to these same stimuli.

Fear developed gradually and appeared in five different manifestations, in the following order: (1) Cessation of the food reaction to stimuli that at first aroused it. (2) A momentary shrinking to the stimulus. (3) Crouching in the nest, consisting of drawing in the limbs and head and hugging closely the bottom of the nest. (4) An attitude of alertness and attentive watching of moving objects. (5) Escape from the nest when approached. These different reactions of fear lasted for unequal periods of time. They also appeared at different ages for the different artificial stimuli used. With the individual bird the transition from one form to the next was very often completely established in twenty-four hours. In general, those artificial stimuli that resembled the natural stimuli most aroused the food reaction longest, but by the seventh day all, excepting occasionally the visual stimulus, ceased to arouse it, and give instead the reaction of fear. The hiss aroused the reaction of fear earlier than any of the other stimuli.

The development of these different manifestations of fear is undoubtedly purely instinctive, affected by association only on the side of inhibition, and in so far as the power of discrimination and associations develop reciprocally.

For the development of behavior as regards the food reaction to the artificial stimuli and the different manifestations of fear, I offer the following interpretation. The first food reactions are purely instinctive, accompanied by little or no power of discrimination, which discrimination remains very crude throughout the period of growth. Discrimination and formation of associations between food and certain stimuli probably develop simultaneously. All stimuli with which no pleasant associations are already formed are then at the same time instinctively feared. Developing fear, therefore, plays a part as well as the formation of associations in the changes in the food reaction to the different artificial stimuli, which is seen especially in the fact that the formation of associations is a gradual process, while the change from one manifestation of fear to the next form is frequently very abrupt. These observations suggest a modification of a part of the present-day theory on the relation of instinct and association in the fear of adult animals, namely, that they come to fear particular things not so much because of unpleasant associations that are connected with them, as because the taming process has not been completed. Their fear for particular things remains in the main instinctive.

The correlation between the three periods in the growth in weight and the periods in the acquisition of motor coördinations holds also for the former and stages in the development of associations and the manifestations of fear. The first crude discriminations and associations are made from the first to the fourth day, and this period marks also the beginnings of instinctive fear. From the fourth to the seventh day there is some improvement in discrimination, and a perfection of associations between food and certain stimuli. With the beginning of the last period of growth there is an abrupt change in all the reactions, the food reaction ceasing for all the artificial stimuli, excepting occasionally for the visual, and fear begins to develop rapidly through several forms of manifestations.

PLATE II.

27.1. Redwing. First stage of motor coördinations.

24.4. Redwing. First stage of motor coördinations.

21.3. Redwing. Second stage of motor coördinations.

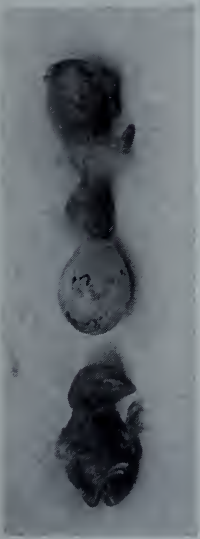
19.2. Redwing. 6 and 7 showing second stage of motor coördinations; 8 and 9 showing the beginning of the third stage.

21.5. Redwing. End of second stage of motor coördinations, showing insufficient neuro-muscular strength for making the acquired coördinated movements effectively.

21.8. Redwing. Third stage of motor coördinations.

17.1. Redwing. Third stage; coördinations at time of leaving the nest.

PLATE II.



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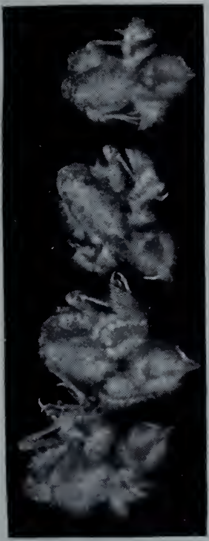

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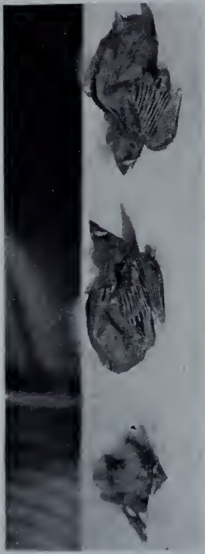

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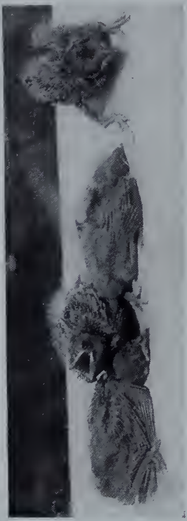

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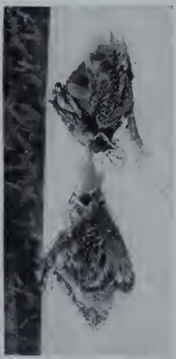

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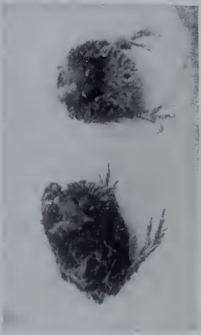

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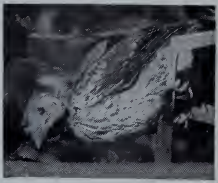


17.1

PLATE III.

35.1. Brown Thrush. Food reaction to hiss.

18.1. Redwing. Food reaction to whistle by 1 and 1; inhibition of reaction by 2 and 2.

32.1. Brown Thrush. Food reaction by all to imitated cluck.

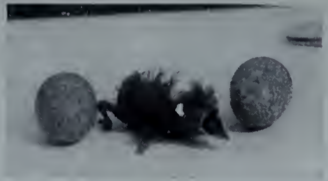
32.3. Brown Thrush. Food reaction to imitated cluck by 4 and 4; inhibition of reaction by 5 and 5.

21.4. Redwing. Food reaction to imitated cluck by 6 and smaller of 7; inhibition of reaction by larger of 7.

22.4. Redwing. Food reaction to approaching hand with extended index and second finger.

PLATE III.

35.1



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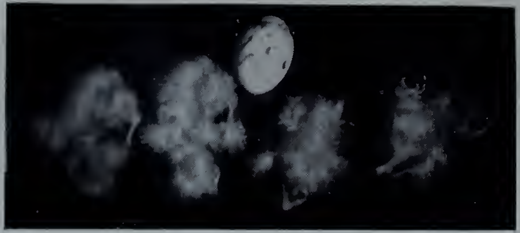

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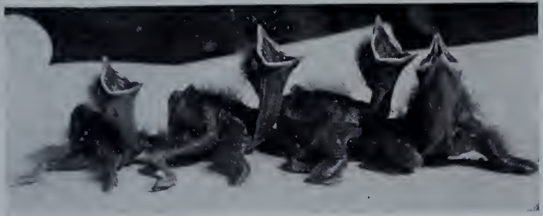

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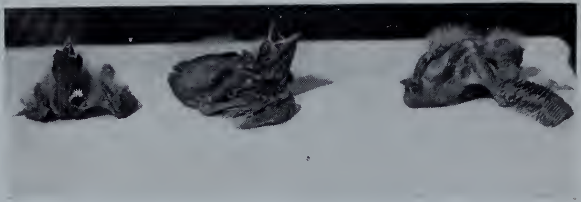

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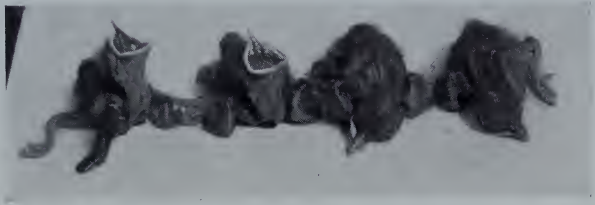

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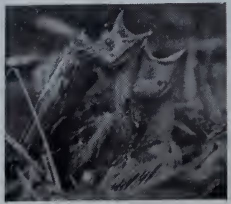

PLATE IV.

32.2. Brown Thrush. Second stage in development of fear. Crouch in nest to hiss.

21.7. Redwing. Second stage. Crouch in nest on being approached.

7.11. Robin. Reaction of crouch in nest combined with reaction of defense (fighting fear) to approach of hand.

7.13. Robin. Reaction of defense to approach of hand.

4.2. Rose-breasted Grosbeak. Third stage of fear. Attitude of alarm in reaction to presence of person and camera.

21.9. Redwing. Third stage of fear. Attitude of alarm to moving hand at camera.

40.5. Mourning Dove. Absence of external manifestation of fear to handling.

38.4. Mourning Dove. Attitude of alarm to presence of person and camera.

PLATE IV.

p. 84

40.5

9

4.2

9

21.9

10

38.4

12

21.7

10 10

7.11

12 13

7.13

15

32.2

4 5

THE DEVELOPMENT OF IMAGINATION IN SCHOOL CHILDREN AND THE RELATION BETWEEN IDEATIONAL TYPES AND THE RETENTIVITY OF MATERIAL APPEALING TO VARIOUS SENSE DEPARTMENTS.

BY STEPHEN S. COLVIN AND E. J. MYERS.

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INTRODUCTORY STATEMENT.

During the winter and spring of 1907 an experiment to determine the ideational type of school children and the development of these types during the school years was performed on five hundred and twenty children in the public schools of Champaign, Ill. The ages ranged from eight to twenty. Later the experiment was supplemented by conducting a similar investigation with about seventy-five students, members of the elementary classes in psychology, in the University of Illinois, and still later with about two hundred such students. These tests were directed towards discovering whether the subjects were predominatingly of a visual, motor, auditory or of a mixed type. The subjects were presented both visually and orally with cards containing angles and nonsense characters, and with nonsense syllables. During some of the tests the motor memory was aided by supplementing the ordinary method of learning by tracing the visually or orally presented words with the blunt end of a pencil on the desk; during others the learning was impeded by the subject holding his tongue between his teeth while studying the words. The memory was then tested for immediate recall and in some instances for recall twenty-four hours later.

A second part of this experiment aimed to discover the relation existing between the various types of imagination and the learning of material appealing to the three chief sense departments (visual, motor, auditory). More especially, the question was asked, "Does the visually minded subject learn more readily material with a visual content, the motor minded material with a motor content, the auditory minded material with an auditory content, or can the visually minded learn as well material with a motor content, etc.?" To test this a simple story, interesting to all grades, was composed which had in it an equal number of words and phrases suggesting the three different types of ideation. The test was here made for both immediate and for delayed recall. A detailed statement of the method and results of these experiments follows.

The tests were all given in the same period of the day, from 9:00 A.M. to 11:30 A.M., and under similar conditions. This period was selected because it was considered to be the time when the children are least fatigued and most liable to put forth their best efforts. The tests were given to the pupils in their own rooms in the school building. The conditions of the rooms were much the same. All were neat, well lighted and ventilated, well governed and had from thirty to forty pupils in a room.

In making the investigation, two things were guarded against. First, care was taken that no suggestions or helps should be given unintentionally by the person making the test; and second, that the pupils should not be fatigued. To avoid errors that might arise from these two sources the writer (M) gave the tests without assistance from the teachers. To overcome the danger of timidity on the part of the pupils, a few moments were taken in which the experimenter sought to gain their confidence.

The condition sought was such as would insure the maximum effort from all. The room was closely watched and in case a pupil did not try, or received help, which was rare, his paper was quietly discarded. The tests were alternated, in such a manner that they would not become tiresome.

I. Résumé of Previous Experiments in this Field.

The determination of the particular ideational type of children has already been made, directly and indirectly, the subject of several investigations in the psychology and pedagogy of the learning process. Among the most extensive studies is that of Netschajeff who tested the development of memory in 687 children in the schools of St. Petersburg. He presented to them various visual objects, tones and noises, numbers, words appealing particularly to the visual imagination (such as pencil, bottle), likewise words with an auditory content (such as noise, song); he further presented words signifying emotions (such as joy, hope, sorrow) and finally words standing for abstract ideas (such as cause and effect). Lobsien repeated these experiments with a modified and slightly improved method, using as subjects 461 children in the schools of Kiel. Netschajeff's subjects ranged in age from nine to eighteen years; Lobsien's from eight to fourteen.

The results of these two experiments agree in general and indicate that the various kinds of memory in their development show periods of fluctuation, the most marked being at the onset of puberty. Lobsien found the greatest relative increase between the ages of ten and twelve. Memory for emotional states remains undeveloped until puberty, then it sets in and grows rapidly, but more rapidly with the girls than with the boys. About the same time begins a rapid development of memory for touch and movement and also the memory for numbers.

Boys in the lower grades have the best memory for concrete objects, for words with a visual content and then for sound, touch and movement. Later come memories for numbers, abstract concepts and emotional states. Girls differ slightly, visual words coming first, then concrete objects, sound, numbers, concepts and acoustic words; later still, touch and movement.

Meumann, commenting on these results and on others obtained by himself and his students, concludes that young children are realists, particularly visualists. The mental processes of the adults are more in terms of the general idea.

S. S. Colvin and I. F. Meyer during the year 1904–05 performed on three thousand school children in the State of Illinois an experiment specifically intended to test the development of imagination. The method employed was quite different from that used by the two previous investigators but the results in many instances agree with the foregoing.

The data for this investigation consisted of compositions written on certain selected subjects, and done as a part of the regular school work. A record was made of words and phrases in these compositions indicating visual, auditory, motor, tactile, pain, olfactory, gustatory and organic images. In the same way the compositions were studied to discover evidence of some of the more complex forms of imagination under the head of scientific imagination, the fairy story, the nature myth, the heroic, the dramatic, the religious and melancholic forms of imagination.

It was found that the four highest forms of mental imagery (for both sexes) stood in the following order: (1) Visual, (2) auditory, (3) motor, (4) tactile. Much lower than these and in the order named came pain images, gustatory, organic and olfactory. The most striking feature of the curves representing the development of imagination is the sudden decline at about the age of puberty of most of the types. This is not true of the visual imagination, however, which has its most rapid rise for both sexes at about the fourteenth year. This growth does not, however, mean that there is a constant increase in concrete visual imagery; it rather indicates that the young person uses words more and more in an abstract and symbolic way.

Pfeiffer investigated carefully for three successive years the ideational types of fifteen girls and found that 44.6 per cent. were visual, 25.3 per cent. were acoustic and 30.1 per cent. motor. Pentschew from his investigations concluded, on the other hand, that for children the motor element forms the basis of memory, while the visual is a secondary factor. Pohlmann thinks that auditory imagery predominates in the mind of the child.

Lay, who conducted extensive experiments to determine the correct method of teaching spelling, concludes that children are especially motor in their tendency and that vocalization is especially prominent in their thinking.

The chief criticism to a large number of these experiments seems to lie in the fact that no thoroughly adequate method has been employed in determining the various memory types for large groups of children. The following experiment has attempted to obviate this difficulty as far as the visual and the acoustic-motor types are concerned.

The second problem on which the present investigation attempts to throw some light, namely, the relation between memory types and the reproduction of material appealing to these various types, has received up to the present time even less consideration by investigators than has the first. An investigation by Segal bears indirectly upon the question. His particular problem was to test memory types in relation to the learning and the immediate reproduction of material. Among other things he concludes that when presentation and learning are in accord with the memory type, the condition for accurate reproduction is most favorable. For example, visual presentation is most favorable to the visual type and acoustic material to the acoustic type.

II. Methods of Determining Ideational Types.

As has already been said one of the great difficulties in the study of ideational types has been the unsatisfactory methods for determining these types. The method to be adequate for determining the memory type of children must be simple and yet accurate. It must be simple and easily comprehended, because the experiment is to be made with inexperienced observers taken in groups, not with persons accustomed to the laboratory and familiar with the use of psychological terms. The importance of this requisite should not be overlooked as many methods produce excellent results when used with trained observers, but are valueless when used with children. The test must also be accurate, that is, it must be such as will indicate at once the actual mental processes of the subject. This is especially necessary in an experiment dealing with children in groups. An adult who has been trained in analyzing his own experience, may be relied upon for his introspections, but the same is not true with children who have had no training. This inability of children to report accurately concerning their mental processes was clearly shown in a series of tests made at the beginning of the present study. A test that involved certain distractions was given to a hundred school children in the fourth and fifth grades. Together with the tests was given a list of questions inquiring concerning the effect of the distractions upon their power to memorize. Of the answers given only a little over fifty per cent. agreed with the actual results received from the tests. The children were not accustomed to analyzing their states of consciousness, neither did they clearly comprehend when asked concerning their experiences. Their answers were merely their idea of what they thought was wanted; not an account of their experiences.

Among the best known methods for determining memory types are the ‘ questionnaire method ’ first used by Galton; the ‘ word method ’ used by Kraepelin; a modification of the ‘ word method ’ used by Secor; Binet's method with the letter squares; and various forms of the ‘ distraction method ’ used by Meumann and others. These are methods devised in most part for the skilled observer and not for children.

The ‘ questionnaire method ’ places much stress on introspection. Two objections to using this method in the present experiment suggested themselves. First, as was said before, children are not capable of accurate introspection.[1] The second objection to the ‘ questionnaire method ’ applies to the device as a method both for the experienced and the inexperienced observer. The presence of imagery depends on different factors, such as vividness of stimulus, recency of the experience, repetition of the observation, etc. In the laboratory these factors are carefully noted in the conditions for experimentation. They are nearly as important as the reports of the observer. In the 'questionnaire method,' these factors are ignored. A set of questions is asked an observer concerning certain experiences, and the one who asks them may know nothing about the time, place or conditions under which the observer came into possession of those experiences. Suppose a list of questions were given a child in the winter time concerning a garden of roses. He has not been in such a garden for nearly a year where he could smell the odor of the flowers, feel their petals and hear the hum of the bees. He has, however, seen such a garden every day as he passed the greenhouse on his way to school. Under these conditions, other things being equal, the observer's report will be more favorable to the visual than to the auditory or olfactory types. The method may or may not be a fair test for memory type. Much depends on the immediate environment.

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[1] Compare in this connection Titchener's criticism of the method in his *Experimental Psychology,* Vol. I., Instructor's Manual, p. 389.

The 'word method' used by Kraepelin is no less difficult from the standpoint of the present experiment than the questionnaire method, nor it is less free from criticism. According to this method the observer is required to write down a list of objects characterized by their color; again a list characterized by their sound, etc., it being assumed that the visually minded person will succeed better with the first test, the auditory minded with the second and so on. It would be nearly impossible to use this method with children in a place where color is everywhere to be seen. However, where conditions are such that the observer would be placed where objects of color could not be seen, the method would still be open to the same criticism as the questionnaire method, namely, the past experiences upon which the observer must draw for his material in the experiment would not be taken into account. For instance, had the observer just visited a park or a picture gallery, he could readily name objects of color. The error is that the method assumes a normal environment where color, sound and motion have an equal part in one's experience, which may or may not be true.

A modification of this method was employed by Secor who presented a list of printed words to the observer, who was to note the imagery that developed in his mind as he glanced at each of the words. This is clearly a method for trained observers and not for school children.

Binet used the letter squares in which, for example, twelve letters of the alphabet were arranged as follows:

P X K B
Y Q H A
F T C V

The observer, after looking at the card with the letter squares for ten seconds, laid it down and attempted to reproduce it from memory, his type of imagery being determined by his introspection. The first part of the method, the reproduction of the letter square, suggests a valuable test for visual memory. Especially is this true in testing the observer's sense of position for the various letters. A modification of this method was used as a test for visual memory in the series finally adopted for this experiment.[2] (See tests Nos. 2 and 3, pages 96 and 97.)

Binet had the observer hold the tongue between the teeth while memorizing syllables in order to impede the kinæsthetic sensations of the speech organs. This was not intended to eliminate motor impressions entirely, but to so weaken them that the loss would be quite noticeable if the observer were strongly of the motor type, the loss being determined by comparing the results of this test with a previous one in which the tongue was free. The method has many advantages over other methods in determining motor memory especially with children. Other forms of the distraction method in which, for example, memory is impeded by having the observer sing or repeat the letters of the alphabet while memorizing the syllables, are too difficult for the average child. Few persons can divide their attention so as to repeat the letters of the alphabet and memorize syllables at the same time. The advocates of this method contend that by constant practice this difficulty may be obviated, the repetition of the syllables

[2] The above described tests together with several others are presented and discussed by Titchener in his *Experimental Psychology,* Vol. I., Instructor's Manual, Chapter XII.

becoming almost automatic, thus eliminating much of the distraction. This may be true with the trained observer, but it is too much to be expected from a group of children. This test was attempted with a class of fifty, who sang the syllable *la,* while memorizing ten syllables. This distraction caused by the noise and the division of attention, was so great that the results could not be relied upon. Binet's method has the advantage in that there is no noise and there is but one thing to attend to.

Meumann advocates the method of distractions (*Methode der Störungen*), on the assumption that the acoustic type is more easily disturbed by auditory stimuli, the visual by visual stimuli, and the motor when the "inner-speech" is hindered.

In connection with this last-named method may also be mentioned the method of aids (*Methode der Hilfen*). Here aids to the various types are used and the type determined by the effectiveness of the aids. This method was used in connection with the method of distraction in determining the motor type in the present investigation.

It has further been noted that the visual type confuses like appearing, but different sounding letters, while the auditory type confuses like sounding but different appearing letters. An attempt was made in the present investigation to apply this as a method of distinguishing between the visual and the auditory types, but the success obtained was so slight and the results so equivocal that it was soon abandoned as impractical.

It has been suggested by several writers that the visually minded person is aided greatly by localization and hence visual memory shows its superiority over the auditory or motor types when such a localization is involved. This principle was made use of in several of the tests made in the present investigation. (See Figs. 1, 2 and 3.)

This principle of localization appears in the recognition of variously placed angles as a test of visual memory. According to this test a series of angles is exposed for the observer to fix in mind. Afterwards, in a promiscuous set of angles, he is expected to recognize the angles previously seen and indicate them my number. The plan assumed that the person having the most distinct image would most readily recognize the angles on the card. A similar plan is to have a series of letter placed at special angles and then exposed to the observer. After exposing the screen ten seconds the observer is to put the proper letters in angles, which are arranged on a card before him. From those two suggestion came test No. 1 in the present series of tests for visual imagery.

Another test suggested for visual memory was to expose nonsense characters or unfamiliar symbols, such as Hebrew letters, the observer later reproducing the same from memory. The objection to this plan is that the characters, since they are exposed for some time, will probably suggest names, and thus the results will be those of auditory and verbal memory as well as visual. This test, however, suggested the nonsense characters, which combined with the idea of spatial relations formed another test of the series for visual memory.

Various other methods for detecting different types of memory were devised, discussed and modified, which together with these already described, resulted in the series of seven tests for ideational types that were used in this study.

III. The Test with Nonsense Material.

For convenience, the series of tests were divided into two groups. The first group was composed of three tests, each of which was intended to appeal to visual memory. They were composed of characters and spatial relations which eliminated as far as possible any auditory or motor aids. The second group contained five tests in which nonsense syllables were used. These syllables were memorized by visual, auditory and motor methods. The tests with their description follow:

Test No. 1.—The children were seated at their desks in an easy position with pencil and paper. On the paper was a series of four letters duplicated five times. They were arranged in promiscuous order in four lines of five letters in a line. (See Fig. 1, *a.*) On a screen, 22 x 28 inches, there were two lines intersecting at right angles, forming four angles. (See Fig. 1, *b.*) In these angles were placed the four letters found duplicated on the papers that were in the hands of the children. The object of this test was to determine who could put the proper angles around the largest number of letters taken in order as they appeared on the paper, in a given period of time. The proper angle was the angle in which the letter was found on the screen. After careful instruction, the screen was exposed to view twenty seconds. As soon as the screen was removed, thirty seconds were given for placing the angles around the letters.

The assumption in using this test was that the strongly visually minded person would carry the image of the relationship between letter and angle with him and that consequently as soon as he saw a letter he would also see the angle it was in from the mental picture. This would give him a decided advantage, and, other things being equal, his power of visualizing could be estimated from his speed and accuracy in placing the angles. This test worked fairly well in the more advanced grades, but was in general too difficult for younger pupils. There was a decided advantage in favor of the rapid working pupil, since speed was largely the standard of judgment, so that often a person might be classed as visual from the results when his success was due to a larger extent to his rapidity of movement and not to the advantage gained from carrying the image with him.

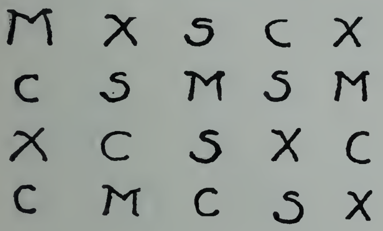

FIG. 1*a*.

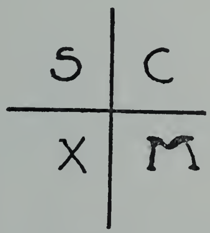

FIG. 1*b*.

Test No. 2.—In the second test, the children were given pieces of paper on which were four right angles with sides respectively parallel and with convenient distances between them. (See Fig. 2, *a.*) This served as a device for the correct placing of characters. On a screen, 22 x 28 inches, was a similar figure and in each of the angles and spaces were meaningless characters. (See Fig. 2, *b.*) These characters were used because they appealed to the eye and at the same time did not readily appeal to the auditory, motor and verbal memory. The screen was exposed five seconds, after which the pupils were to reproduce these characters, putting them in their proper place. In other words, the pupils were to make their paper look like the screen. The short time of exposure allowed very few to associate names with the characters, not more than one in five according to introspections that they gave in answer to questions concerning the naming of the characters. This test, on account of its simplicity and ease of grasp, was of especial value to the children of the lower grade. It was a test which appealed to all alike and there was no misunderstanding as to what was expected. In recording the results, three different methods of estimating their accuracy were employed; one on the numbers of characters attempted, one on the number of correct characters, and a third on the number correctly placed.

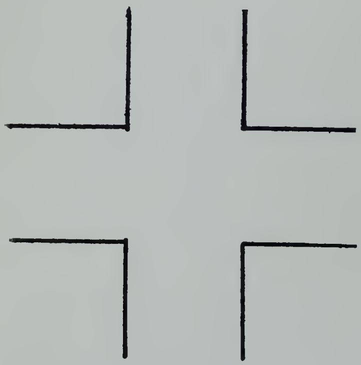

Fig. 2*a*.

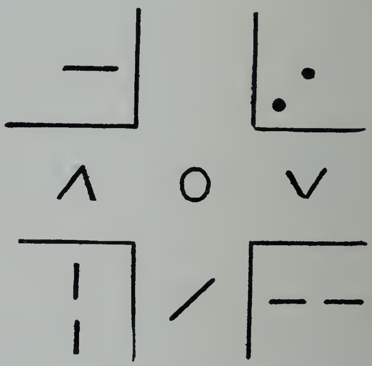

Fig. 2*b*.

Test No. 3.—The third test of this group was similar to the second. The pupils were provided with papers ruled like the first. This time a screen was exposed which had letters and figures in the spaces instead of nonsense material. These characters were employed that all might have the use of sense symbols. These letters and figures were of different forms. Some letters were capitals, some were small, some were script and some were printed. The figures were Roman and Arabic and all characters had a specific place. Ten seconds were given for observing, after which thirty seconds were given for reproduction.

This test, though more complex, was used as a complement to the second test. In the second test no one was supposed to name the characters, though there was nothing to prevent some name being attached to them. In this test, all the characters had names familiar to all. The letters were black, bold and of different size, while the angles were red, as was the case in the second test. The form and spatial relation were the dominating elements considered. There were three grades given, one grade on the number of characters correct, one grade on the number of characters having the exact form in which they were written and a third grade on position. By a comparison of the results here obtained with those of the second test, a fair idea of a person's visual powers might be had.

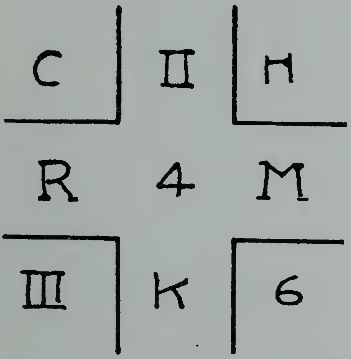

FIG. 3.

In the second group of the series, four tests in number, nonsense syllables were used. The results of this group were quite satisfactory. The tests were clear, easily understood and but slightly different from the regular school work. The following are the tests of this group given in their respective order, but numbered as they occurred in the series.

Test No. 4.—Upon a piece of white cardboard, eight by twenty inches, was placed, one above the other, a row of six nonsense syllables. These syllables were composed of three letters (two consonants and a vowel) and were made of black gummed letters, one and one-fourth inches high. The children were seated at their desks with pencil and paper. Upon a given signal the screen was exposed and twenty seconds were given to commit the syllables. At the close of twenty seconds the screen was removed and the children asked to write the syllables. As this was the first test with nonsense syllables, it was after a few moments repeated with different syllables, and the average of the two was taken as the initial test for the group.

Test No. 5.—In the fifth test a screen similar to the fourth was used but with different syllables. The only difference was in the manner of memorizing. As soon as the screen was exposed, the children took the blunt end of their pencils and wrote each syllable on their desk once. After this they were allowed the remainder of the twenty seconds to memorize the syllables as they chose. By comparing tests No. 4 and No. 5 some indication of the motor memory of the forearm was given, but not necessarily for other parts of the body.

Test No. 6.—After a rest from the first two tests, a third was given. The conditions were the same as in No. 4 and No. 5, and the same test was used only with different syllables. This test was to determine the value of the movements of the lips, tongue and throat, to the memory. In test No. 4, which was used as the standard, these organs were allowed absolute freedom in the process of memorizing. In test No. 6 these movements were impeded by catching the tongue between the teeth and holding it there during the test. Care was taken that the tongue was well extended and held both while learning and writing, so that there was little aid from this source. This method did not completely remove the motor sensations of the speech organs, but it did so impede them that any one strongly dependent on these sensations as an aid to memory could be readily detected. The results from this test compared with those of the initial test gave evidence of the extent to which the memory was aided by the sensations from the speech organs.

Test No. 7.—The seventh test was intended to appeal to the auditory type. While it is by no means certain that a person will employ auditory imagery in remembering syllables given in auditory presentation (it being quite possible for him to translate them directly into motor or visual symbols), it is fair to assume that the person who is distinctly of visual type will be somewhat at a disadvantage in remembering material thus presented. For persons of the motor type, the disadvantage will not be so great; still the distinctively auditory type should succeed better than the motor in such a test. The six nonsense syllables were used as before but this time were read and not exposed to vision. These syllables were pronounced and spelled slowly and distinctly to the pupils and then repeated. This took twenty second, the time of the other tests of this group of the series. At the close of the second reading the syllables were reproduced. Here the visual elements were at the minimum and the auditory at the maximum.

IV. The Test with Sense Material.

The series of tests described above dealt largely with material without meaning. The matter of interest was eliminated, and association was reduced in effectiveness. The test now to be described employed sense material appealing to interest and involving associations of considerable complexity. The purpose of this part of the investigation, as has been said above, was to discover, if possible, whether a given ideational type tends to retain better material suited particularly to this type, or whether the nature of the material is a matter of indifference. The test consisted of a story constructed with the purpose of appealing in its various parts to the three chief types of mental imagery (visual, auditory, motor). It was further constructed so as to gain as far as possible the attention and interest of the pupils of the various grades in which it was presented. This story was as follows:

Walter Brown, a young man who lives in Trenton, N. J., had an exciting time one night a few weeks ago.

A short time after he had gone to bed and when he was

A–1
just about falling asleep, he heard the (fire whistle blow loud
A–2
and sharp); then an (engine and hose cart came dashing over
A–3
the pavement) followed by a (crowd of shouting boys). Soon
A–4
it was (quiet again and for a time very still); then he heard
A–5
(the slow tramp, tramp, of the policeman) on the beat and
A–6
listened for the (ring of his club as he struck it against the iron
lamp post) at the corner. Mr. Brown was almost asleep again,
A–7
when he heard (the sharp clang of the motorman's bell) on the
A–8
street car next block; then the (big clock down town struck
A–9
with its deep tones). A minute later there came (a long, low
A–10 A–11
whistle), (a step on the porch), then (a gentle tap, tap, tap),
A–12
right under his window and then a (scraping sound) and (a
A–13 A–14
whisper) and (the noise of falling glass).

M–1 M–2
Mr. Brown slowly (raised himself in bed), (bent forward)
M–3 M–4
(and listened). Then (he carefully put one foot out of bed),
M–5 M–6
(held it a moment) and then (stepped softly on the floor).
V–1 M–7
(It was very dark), (and he felt his way with his hands
stretched out to the door of his room).

M–8 M–9
Then he (slowly crept down stairs), (holding his breath).
M–10 M–11
He (reached the lower hall) and (quickly turned the button)
V–2
that threw on the electric lights. In a moment (all the rooms

V–3
were lighted), and (he saw standing near the door of the
V–4 V–5
dining room) (a man with a red beard), (a black mask, partly
V–6
covering his face). The man (wore a blue coat), (and dark
V–7 V–8
trousers, spotted with mud). He had (a yellow cap on his
V–9
head), and (in his right hand he carried a revolver) (that
V–10 V–11
glistened in the light). (In his left hand he had a green
V–12
bag), and on (the side-board was a heap of silver spoons just
A–15
ready to be taken away). The burglar (gave a cry) and
V–13 V–14
(turned off the lights). (It was totally dark again). (Then
M–12 M–13
he sprang at Mr. Brown) and (pushed him to the door).
M–14
Mr. Brown (tried hard to keep from falling) and (held the
M–15 M–16
burgler tightly) around the waist. (Then he struck at him
A–16
with his fist), (and cried out help, help as loud as he could).

V–15
(Suddenly the lights were turned on again), and there were
V–16
(two policemen standing in the hall way with their revolvers pointed at the burglar). So Mr. Brown's midnight adventure ended and he escaped unharmed.

The parts of the story enclosed in parentheses are those portions or elements intended particularly to appeal to the three principal ideational types. Each parenthesis contains one such portion. The capital letters A, M, V, indicate respectively those portions appealing to auditory, motor and visual imagery, and the numerals after each letter show the number of these elements for each type. It will be noticed that there are sixteen for each type, forty-eight in all.

The results were estimated in terms of the retentivity of the subject for each of these elements and they were then correlated with the results of the first set of tests in such a way as to show the retentivity of the visual type for the three kinds of material and in a like manner of the auditory and motor type of subjects.

The story was given to the subjects in auditory presentation. The test was carried out under conditions similar to those under which the first series of tests were conducted. It was in the same rooms with the same pupils and at the same time of day. The parts of the story appealing to different types were emphasized with equal prominence. No comment was made on the story and no explanation made. The pupils were told the story was to be read to them and after it was finished they were to reproduce it, telling everything that was in it.

Immediately after the story was read the pupils wrote it out on paper that had previously been provided them. The papers were collected, marked and filed. The next day at the same time, a second reproduction of the story was taken. There had been no previous warning that this was to take place, nor were there any suggestions given concerning the story. These papers were likewise filed for grading. Two weeks later, at the same time of the day, a third reproduction was taken in the same manner.

These papers were graded and the grades recorded in a manner similar to that used in the first series of tests. Each set of papers was graded separately and afterwards the papers were rearranged so that the papers from each person were together and the grades recorded. Each paper had three distinct grades, one for visual memory, one for auditory and one for motor.

V. Analysis of Results.

The following two tables give a summary of the classification of the various ideational types as obtained by the first series of tests:

TABLE I.

	3	4	5	6	7	8	H	U
Visual	72	67	59	49	47	42	46	50
Auditory...	16	37	57	49	46	57	46	30
Motor	20	29	24	27	34	20	23	26
Balanced ...	20	20	10	21	19	24	28	20

TABLE II.

	3	4	5	6	7	8	H	U
Visual ……	58	51	45	24	26	23	31	45
Auditory…	11	11	40	36	34	32	31	18
Motor…….	11	18	5	19	21	21	10	17
Balanced …	20	20	10	21	19	24	28	20

The above results were obtained by two different methods of classification of the data secured by the experiments. In the first table all subjects classed as partially visual, auditory or motor are represented under the appropriate type as indicated at the left. In the second table those classed as predominatingly of a particular type are classified under that type alone. For example a subject might be classed as visual-auditory. In the first table he would be represented in both the visual and the auditory columns, but in the second table he would be represented in the visual column alone. If the subject were on the other hand classed as auditory-visual, he would be recorded in the first table under both the auditory and visual groups; in the second table only under the auditory group. The numbers accompanying each group represent the per cent. of subjects classified under that group. Since in the first table a subject may be classified under two groups the total of each column is in every instance greater than 100. In the second table, since a single individual can be classified only under one group the total for each column is exactly 100. The numerals at the head of each column indicate the school grades from the third to the eighth inclusive. The letter H indicates the results for the high school students, and U the results for the university students.

By studying these two tables, the reader can see at a glance the preponderance of the visual type in the lower grades, and the gradual falling off of this type as the pupil advances in the grades, with a tendency to recover somewhat in the high school and university. The same fact is shown by another method of representing the results obtained in the first series of experiments.

These latter results are embodied in the curves of Fig. 4 found on page 104. These curves were obtained by plotting the averages of correct results of the various tests in the first series, for the different grades and for the high school and the university. Curve *A* was obtained by averaging the results of tests No. 2 and No. 3, both of which were devised to test visual memory. Test No. 1 was not included in these averages because of the fact that the results were equivocal and on the whole unsatisfactory for most of the grades and particularly so for the university. Curve *B* embodies the results of the test with the nonsense syllables visually presented; curve *C*, of the nonsense syllables visually presented and supplemented by tracing the syllables when exposed to view; curve *D*, of the nonsense syllables visually presented to the subject while he was inhibiting this tendency to vocalize by holding his tongue between his teeth, and curve *E*, of the nonsense syllables with auditory presentation.

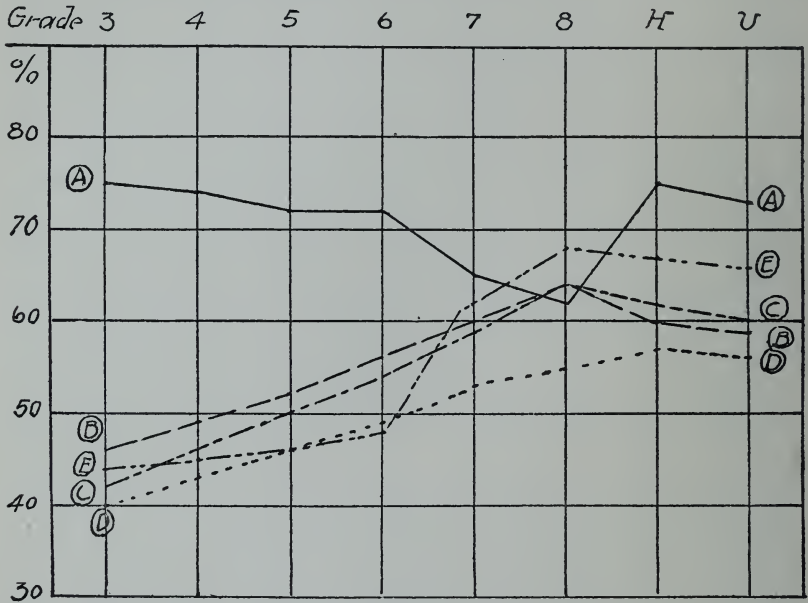

Fig. 4.

It may be easily seen by this curve that the concrete visual imagery as indicated by the second and third tests is remarkably strong in the earlier years. In the third grade the pupils made an average of seventy-five per cent. of correct results with these two tests; in the fourth grade of seventy-four; in the fifth of seventy-two; in the sixth grade of seventy-two; in the seventh of sixty-five; in the eighth of sixty-two; in the high school of seventy-five; and in the university of seventy-three. In other words, notwithstanding the fact that throughout the grades, the high school and the university, there is an ever-increasing ability to remember,[3] nevertheless the results of the third grade are higher in correctness than those of any other, with the single exception of the high school, in which an average of 75 per cent. of correctness was also obtained. The results of the tests in the university are two per cent. lower in correctness than those of the third grade for this test. These results clearly indicate an actual decline of concrete visual imagery during the school years.

On the other hand it appears from Fig. 4 that there is a fairly constant increase in verbal-visual memory throughout the years studied. This fact is particularly indicated by curve *B* (compiled from the results of the test with nonsense syllables visually presented). This curve begins in the third grade at 46 per cent. of correctness and rises in the main, being practically constant in the higher grades. In the lower grades it seems probable that a large part of the successful results with this test were obtained by the subjects holding a concrete visual image of the letters in their minds, while in the higher grades the *significance* of the syllables was the important element in retention. Undoubtedly here the visual imagery was assisted by the motor and auditory. The fact that the concrete visual imagery is strong in the lower grades and that there is a general tending toward the growth of a verbal type is also shown by certain facts noted incidentally during the course of the experiments.

In the earlier grades low averages were more often the result of the omission of characters than of an error in form or in place. In the higher grades, on the other hand, errors in form and place were more frequent, while omissions were far less. In these grades too an attempt was frequently made to assign names to the meaningless characters, a procedure reported less often in the lower grades. In the fourth grade thirteen per cent. of the pupils reported that they had attempted to name the characters, while in the seventh grade there were thirty-five out of a hundred that so reported. In the university it was the general procedure to assign some name to the characters, and to join the nonsense syllables together in some meaningful way.[4] It seems quite evident from these results that the undeveloped mind relies primarily on the impression for retaining its material while the more mature mind is greatly aided by holding the material through associations. Burnham, in his article on 'Retroactive Amnesia,' has pointed out the significance of the two elements in memory, the impression (*Einprägung*) and the association. The work of Müller and Pilzecker has clearly established the existence of the *Perseverationstendenz* as a factor in memory, and these results have been verified in other connections. This all goes to show that apart from the association of memory material by which it is retained and called up, there is also the factor of persistence that is due rather to the impression than the association. This sheer persistence seems to be a more important factor relatively in states of fatigue and mental disturbance than in normal conditions.[5]

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[3] The results of Bolton, Bourdon, Hawkins, Winch, Binet and Henri, and others on the growth of memory in children have clearly established this fact. Meumann says that the adult learns various kinds of material in a much shorter time and with less repetitions and with less fatigue than do children. Young children, however, retain more exactly.

It is likewise reasonable to assume that it is a more important element in the mind of the young child than in that of the more developed pupil. It is natural therefore to expect that concrete imagery will be more in evidence in the lower grades than in the higher, and since visual imagery is the most vivid and real of all imagery that it will be the predominating type among young children. This expectation the results of the present experiment seem to sustain.

The fact that mere impression is a more important element in the recall of young children than of older is further shown by the fact that where the nonsense element is the most marked

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[4] An analysis of results seem to indicate that ninety per cent. of the university students did this to a greater or less extent.

[5] Heilbronner and Stransky working on abnormal subjects both assert that 'perseveration' is something that forces itself into the regular train of association, and it to be treated as an accident (*Ausfallerscheinung*).

(namely in tests No. 2 and No. 3) there is, as has been pointed out, an actual decline in correctness. This is not the case with the tests involving the nonsense syllables which permit a certain amount of association. Tests Nos. 4, 5, 6 and 7 all show some, though not great improvement in correctness as the age of the pupils increases.

Further, the steady increase of memory throughout the grades for visual auditory and motor material alike, when this material is connected in meaningful associations is shown in the results with the story. (See Figs. 5 and 6, page 109.) Here there is an increase in the per cent. of correctness in immediate recall for the visual elements of the story from twenty-four per cent. in the third grade, to seventy-four per cent. in the university; in the auditory element from twenty-three to sixty-nine per cent., and in the motor from forty-seven to sixty-eight per cent. The corresponding figures for the recall one day later (Fig. 6) are: Visual imagery, for the third grade twenty-four per cent., for the university seventy-four per cent.; auditory imagery, for the third grade twenty-two per cent., for the university sixty-six per cent.; motor imagery, for the third grade forty-four per cent., for the university seventy per cent.[6]

The marked decline of curve *A* representing the results of tests No. 2 and No. 3 in the seventh and eighth grades and its recovery in the high school and university is possibly to be explained as follows:

The chief element in the retention of the younger children is the impression represented by the concrete visual image; of the more mature child and the adult, the association. The rapid decline of the concrete image is not compensated for by a corresponding increase of the element of association until the high school and the university are reached. There is, therefore, an actual decline in memory for concrete visual material at about the onset of puberty. This decline has already been pointed out, as before stated, by previous investigators.

If we turn again to the tables on pages 102 and 103, we will notice the growing importance of auditory imagery as determined by test No. 7. This test shows the relatively slight importance of this type of imagery in the lower grades and its growth in the last four years of the grammar grades. A somewhat similar development is shown by curve *E*, of Fig. 4. Here, however, the pronounced development of the auditory type is indicated as first manifesting itself in the seventh and eighth grades. How much the increase represented in the tables and in the rise of curve *E*, is due to a change in ideational type and how much of it is due to the fact that auditory presentation is more favorable for learning in the higher than in the lower grades is, however, a question.[7]

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[6] The preponderance of the motor element of the story in the recall in the lower grades is not out of harmony with the seemingly contradictory results obtained with the nonsense material. Its meaning will be discussed later.

[7] A number of investigators have attempted to determine which mode of presentation (visual or auditory) gives the better result in the recall of the material presented. Hawkins, who presented visual and auditory words at the same rate to his subjects, concluded that auditory presentation was superior for children; visual for adults. Kirkpatrick found younger pupils superior in memory of spoken words. On the other hand, Miss Calkins found visual presentation superior to auditory. MacDougall, using nonsense syllables, agrees in this. Whitehead, also using nonsense syllables with adults, found visual presentation better. The most enlightening results in this field are by Pohlmann who found acoustic presentation better for significant material, visual for nonsense material. The fact is of special importance in connection with the results of test no. 7, which has to do with nonsense syllables. In the lower grades the nonsense syllables cannot be imaged well when pronounced to the pupils because they are unfamiliar. In the higher grades this significance can more readily be comprehended and therefore they do not put the older pupil to the disadvantage under which the younger suffers.

In this connection it may be well to point out the probable reason why the younger pupil more readily retains material that has meaning when presented in an auditory manner than in a visual. It is not because the younger pupil is of a less pronounced visual type than is the older pupil. Indeed the results of this and other studies show the reverse to be true. It is rather because auditory presentation is of necessity successive and visual generally simultaneous that the superiority of the auditory over the visual is shown. The young pupil attends better when he gives successive attention to the material presented in an auditory manner than he does to the visual material over which his eye may move at will and without plan. If he is visual minded the material presented in an auditory manner is quickly (for the most part unconsciously) translated into visual terms and what is lost in this translation is more than compensated for by the more effective attention secured through the successive presentation when the material is read to the pupil. With nonsense material, however, the visual presentation is a tremendous advantage in the earlier grades over the auditory presentation and the gain in successive auditory presentation is more than counterbalanced by the loss which the pupil suffers in not being able to form a distinct image of the spoken nonsense syllable.

It is probable that both the manner of the presentation and the type of the pupil are factors in the general result. Some further light is thrown upon this question when we examine the curves in Figs. 5 and 6. These curves, as has been previously stated, give the general results of correctness in the reproduction of the three different elements of the story. Here

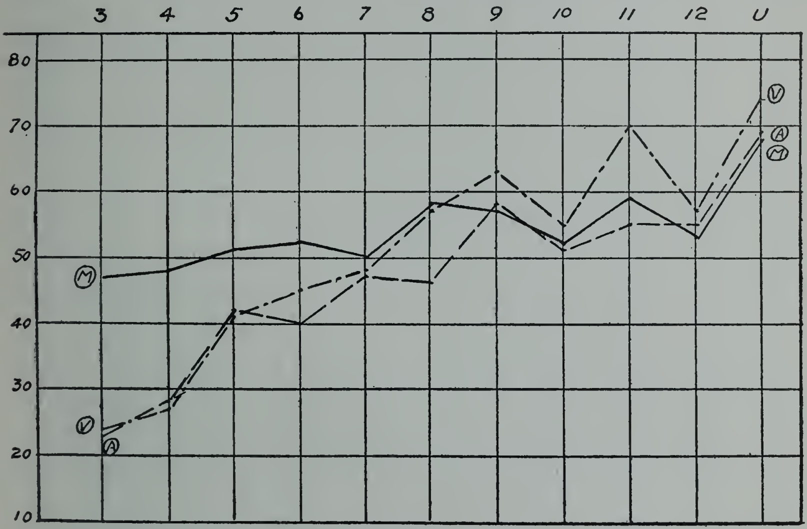

FIG. 5.

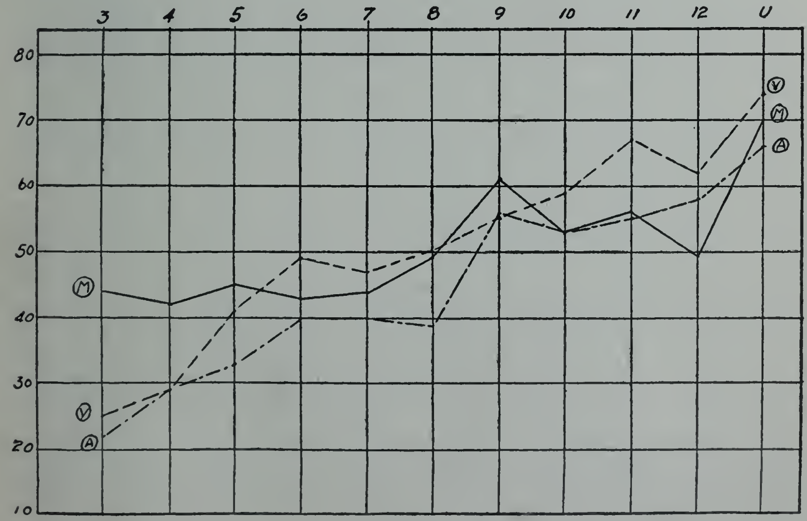

FIG. 6.

the manner of presentation is a negligible factor since it is throughout auditory. From these curves it will be seen that the auditory element is in general inferior to the visual.

On the whole it seems reasonable to conclude that the auditory type approaches maturity later in life than does the visual and it is probably much more closely associated with the motor, which as can be seen by the tables and by the curves of Fig. 4, is in general inferior to both visual and auditory. Probably both motor and auditory imagery are more closely allied with verbal imagery. There seems less tendency for the motor type to grow as the age of the pupil increases than for the auditory type. The curves of Fig. 4 show the slight importance of this type in recall. Curve *B*, as has been said, gives the results of the test with visually presented nonsense syllables; curve *C* of the visually presented nonsense syllables when the learning was accompanied by tracing the syllables seen, with the blunt end of the pencil, and curve *D* of the visually presented syllables learned and reproduced with the tongue between the teeth. It is a surprising result that the tracing is not an aid to the learning in the earlier grades. Indeed it is a hindrance until the eighth grade is reached. This is to be explained by the fact that the movements are an actual distraction and that the motor imagery is of so slight importance that the distraction is more of a hindrance to learning than the motor imagery is an aid. The relatively slight importance of the motor imagery of the speech organs is shown by curve *D*. While this curve is lower than curves *B* and *C* the difference is not great, and that difference may probably be attributed more to distraction than to dependence on motor imagery. It is slightly more pronounced in the upper than in the lower grades, however. This may indicate the increase of the ‘inner-speech’ as the age of the pupil increases.

The results from the motor tests were strikingly different from what might be expected from children in the lower grades. In the tests at this period action was strongly manifest. There was scarcely a child that did not move his lips and movements of the entire body were not infrequent, especially when interest was great.

In the fourth, fifth and sixth grades, especially in the last two, there seems a strong tendency for the child to attempt to employ his motor impressions to assist in memory. To do this he does not depend much on particular muscle sensations, but all the body seems to be active. When intensely interested he moves his arms, twists in his seat, strikes his fist into the palm of his hand, shifts his feet and goes through a variety of other movements, but does not use his pencil. No doubt he tries to bring these movements, in a general way, into coördination with his visual powers, but only in the most indefinite way. The muscles of the fingers are not used, as in the higher grades. It is quite probable, however, that if all the random movements had a direct bearing on the material in hand they would be of much value, but they are too general to be effective to any great extent.

In all the grades below the high school the lips were almost invariably used in memorizing syllables. So conspicuous and so common is this practice that it has been urged by educators, that lip movements should not be inhibited during study. Not ten per cent. of the pupils in the grades kept their lips motionless in the course of a test with syllables. Yet the actual assistance received from the motor sensations in the early period is not great. It is often over-estimated. It is probable that the motor impressions are not used during this period as an aid to memory. While the child moves much, the movements are not such as to be of value to the memory process. The child writes comparatively little before the sixth grade, and the use of the lips as a means of fixing impressions on the mind is probably more or less automatic. Other motor responses accompanying the learning process are general in character and so many and so diverse that the mind loses sight of them as a means of assistance. About the sixth grade, or seventh grade, writing becomes of much more importance as an aid to memory.

There seems to be some difference of opinion among investigators in regard to the value of motor aids in memorizing. Lay concludes from his experiments with children, to whom he presented both visually and orally nonsense words under different conditions, that these aids are important. Itschner, who repeated Lay's experiments under more careful conditions, does not agree that the favorable effect of writing the words presented is due to the motor processes, but to factors of greater attention, etc., accompanying this procedure. Fuchs and Haggenmüller who worked with unfamliar Latin and German words, found on the whole that learning without vocalization or in a whisper was superior to pronouncing aloud. Writing in the air was a great hindrance, while copying and pronouncing in a whisper was an even greater distraction. Pohlmann says that the addition of the motor element is not favorable to memorizing. Miss Métral, in a recent study with boys and girls from seven to nine years, concludes that speech and arm-motor aids are valuable in learning to spell. The studies of Smith, of Cohn and of Fränkl also seem to indicate the value of motor aids in memorizing. (For reference to these three last named investigators see the bibliography at the conclusion of the fourth study in this monograph. The results of Lay, Itschner, Fuchs and Haggenmüller are discussed at some length in this latter study.)

The results discussed above show the way in which the mind develops rather than the cause for the particular tendencies in this development. Evidences are not lacking, however, to show that training[8] has much to do with the development of particular types of mind.

In the third and fourth grades the types were more distinctly marked. There were much wider individual differences in the results of tests. In these lower grades there is extreme variety in results, which is not so marked in the higher grades. While vision holds the predominance over other types, yet it was not an infrequent occurrence to find a person who showed very strong auditory-motor type with poor visual powers. The circumstance that the visual power in general is strong at first is due to its being the first type of imagery to be effectively used and in the earlier years it is constantly being developed. In these early stages vision seems the natural avenue for gathering information and in this it has the advantage in the outset, which is shown in the first years of school life. From the fourth grade on to the eighth there is a growing tendency toward what might be called a balanced type in which the three types are more on a level.

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[8] In this connection the discussion of Segal in regard to memory types should be mentioned. He contends that these types are not so fixed and characteristic as the work of Charcot and other later investigators might lead us to suppose. The particular type that an individual possesses for a certain class of objects depends largely on the customary nature of the presentation of that class of objects; and it is not to be wondered at that the visual type is predominant when we remember that most of the objects to which we attend and which are of importance for us, come through visual sensations.

As the pupil advances in the grades his attention is more and more taken from concrete visual objects and centered on reading and writing,[9] the auditory and motor sides thus being especially emphasized.

Before passing on from this part of the investigation, it may be worth while for us to consider a seemingly surprising fact that came out incidentally in connection with certain results, and which, while not being an integral part of the experiment itself, has an important bearing on the general problem of the psychology and pedagogy of learning.

During the course of the first part of the experiments in several instances, the tests with the nonsense syllables were repeated twenty-four hours later with the result that only a very slight falling off in the recall over the previous day was indicated. As this result was obtained toward the close of the series and as time did not permit an extension of this aspect of the experiment, not a sufficient amount of data was obtained in this connection to make any extensive generalizations.[10] The results of the second part of the experiment (the test with the story) were, however, even more striking in this particular and showed clearly that there was practically no loss in the recall twenty-four hours after, as compared with the immediate recall. By comparing Figs. No. 5 and No. 6, the reader can see that the falling off in memory after twenty-four hours is extremely slight. This fact can perhaps be better seen by glancing at the following table in which the figures at the left in each column indicate the per cent. of correctness for each grade in immediate recall of the story, and the figures at the right the per cent. of correctness for the recall twenty-four hours later.

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[9] An extensive series of experiments recently concluded in the Urbana, Illinois, schools, show that the process of writing is not fully mechanized before the seventh or eighth grade.

[10] In the Urbana tests referred to above, and which were made with series of nonsense syllables, the average for the five upper grammar grades was as follows: Immediate recall (three different series of tests, 61.55 per cent.; 48.6 per cent.; 48.3. Recall after one hour, 56.9; 46.1; 50.1.

	3	4	5	6	7	8	9	10	11	12	U
Visual Elements	24–25	27–29	41–41	45–49	48–47	57–50	63–55	55–59	70–67	57–62	74–74
Auditory Elements	23–22	28–29	42–33	40–40	47–40	46–39	58–56	51–53	55–55	55–58	69–66
Motor Elements	47–44	48–42	51–45	52–43	50–44	58–49	57–61	52–53	59–56	53–49	68–70

It can be seen from an inspection of this table that in the third and fourth grades the per cent. of correctness for recall after twenty-four hours is practically as high as that for immediate recall. This is likewise true of the high school (grades nine, ten, eleven and twelve) and for the university. Grades five and six show a somewhat more marked falling off in correctness after twenty-four hours than do grades three and four, and in the seventh and the eighth grades this difference is quite pronounced. The fact that the loss is the greatest at this latter period may possibly be explained in the same way as the decline in curve *A* (shown in Fig. 4, and representing tests No. 2 and No. 3 in the first series) was explained, namely, by the fact that the concrete imagery which is strong with young children, has become weakened in the latter years of the grammar grades, and that power of association has not at this time sufficiently developed to make good the loss of the concrete imagery. This slight falling off in the curve of forgetting after a period of twenty-four hours when a much greater decline would have been expected, has been noted by several investigators.

It will be recalled that Ebbinghaus in his pioneer experiments with nonsense syllables found that, tested by the *Ersparnismethode,* the rate of forgetting was proportional to the logarithm of the time. These results were later confirmed in the recall of tones by Wolfe and others. On the other hand Boldt, in an experiment with thirteen normal and thirty-five pathological subjects, found that for the former an increase of the interval between the presentation and the recall resulted in an increasing accuracy. Reproduction after five minutes was not as good as after fifteen, and the best results of all were obtained after twenty-four hours. The fact that these results held good for normal subjects and not for those suffering from mental disease is significant and will be referred to again.

Lobsien likewise found in experimenting with twelve pictured objects that the repetition of the test without intervening recall indicated that memory in children increased in accuracy for from one to two days, and Binet reports that many scholars, eight days after learning, remembered more than immediately after. Experimenting with smaller intervals, Finzi found that the best reproduction does not come immediately after presentation, but somewhat later (from six to thirty seconds).

The explanation offered by Watt in commenting on Lobsien's experiment, that the children talked the matter over in the meantime, may in part explain some of these seemingly strange results, but is hardly sufficient to account for them as a whole.[11]

The discrepancy between the results of the present experiment and those of Ebbinghaus and of Wolfe may in part be explained by the fact that a different method of testing the recall was used by Ebbinghaus and by Wolfe than by the writers of the present paper. Ebbinghaus used the *Ersparnismethode,* as previously stated, and Wolfe the method of identification, while in the present experiment memory was tested by the amount retained in the recall. These differences in the method of testing the memory are not, however, sufficient to explain the great difference in the results. Ebbinghaus used nonsense syllables, Wolfe tones, while in the present experiment sense material (the story) was employed as a test for delayed recall in most instances, although as previously stated, a few results were taken with nonsense syllables.

[11] The tests in the Urbana schools were so conducted that the delayed recall excluded the possibility of communication in most cases.

The most reasonable explanation for the difference of the results in the experiments of Ebbinghaus and of Wolfe and in those of the present investigation is to be found in a fact already discussed, namely, that two factors are necessary for effective recall, namely, the impression and the association. The tones used by Wolfe offered but few opportunities for association and hence the chief means of recall was through the impression, which was probably not particularly vivid, especially for adults who had lost to a great extent their power of concrete imagery, if indeed, they had ever possessed it, for tones. Naturally then, there would be a somewhat rapid falling off in memory under such conditions. Ebbinghaus used himself as observer in his test with nonsense syllables. These he had trained himself to learn without bringing in tricks of association, something which the untrained observer has a constant tendency to employ. Ebbinghaus must, therefore, have relied largely in recall upon the impression and not on the association. Hence came the rapid falling off in the memory curve. Both with nonsense syllables and with the story in the present experiment there was doubtless a large rôle played by association in the recall, and as Dr. Burnham has pointed out in his article (previously cited in this paper) association requires some time to fix itself. Hence the length of the interval was an important factor in the accuracy of recall. In this connection the results of Boldt may again be cited. He found with normal subjects that an interval between learning and recall strengthened the memory, but this was not true with pathological subjects, whose associative processes were much weaker than those of the normal subjects. The pathological subjects were not able to retain the material presented, because they could not associate it. Goldstein, working also with pathological subjects, found a marked distinction between the *Einprägung* and the association. The subject might be successful in immediate recall, while showing great weakness in delayed recall.

It must be remembered that in the case of young children, while the associative element is not so important in recall as with the adult, the impression of the sense material is doubtless much stronger, and it is preserved in concrete visual imagery for some time. It, therefore, does not fade from the mind as rapidly as the same sort of an impression would fade from the mind of an adult who has lost his power of concrete imagery to a great extent. For these reasons it is hardly to be expected that the memory curve of Ebbinghaus would hold good under the conditions of the present experiment, either for the child who relies largely on the vividness of the impression, or for the young person and adult who rely much more on association. Granted that the results of the present experiment are valid in the point now under discussion, these results by no means disprove those of Ebbinghaus; they do, however, indicate that his results have a much more limited application than has generally been supposed, and that they are not valid in general for learning under the usual conditions of the school.

These results suggested a parallel between the growth of memory and the acquisition of a skilful act by an individual. The study of Bryan and Harter in the learning of the telegraphic language, and other studies later made along similar lines, have shown the fact that plateaus develop at certain stages of the process of acquisition, and the individual must wait for a new set of coördinations to be perfected before an advance to a higher level can be made. It is true that here there is not actual loss, as there is when the concrete imagery becomes weakened in the child's mental development. On the other hand both in the development of memory and in the perfection of a skillful act an advance is conditioned on the perfection of new powers as yet imperfectly under control.[12]

The last part of this discussion brings us to the question of the relation between the various memory types and the learning of materials appealing to the different sense departments. As has already been said, but little attention has been paid to this problem by investigators. The work of Segal, which bears somewhat directly on the subject here under discussion, has already been mentioned. Meumann agrees with Segal, that it is advantageous to fit the material presented to the ideational type of the learner, and not as Münsterberg and Bigham maintain, present the material through all possible avenues. In other words Meumann believes that the visually minded should as far as possible have the material presented so as to appeal to the visual type of learning, the auditory to the auditory type, and so on. The present experiment attempts to throw further light on this problem by studying the relation between the retention of the various elements in the story and the three idea-

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[12] In this connection my colleague, Dr. J. W. Baird, suggests an interesting fact in the learning of the game of billiards. He informs me that the first shots made by beginners are the more simple carrom shots. Later when the cushion shots are tried, the ability to make the carrom falls off, and the novice, who has not as yet perfected his cushion shot sufficiently to make it of any great value to him, actually falls off in his play for the time. Thus there is a decline in the curve of efficiency.

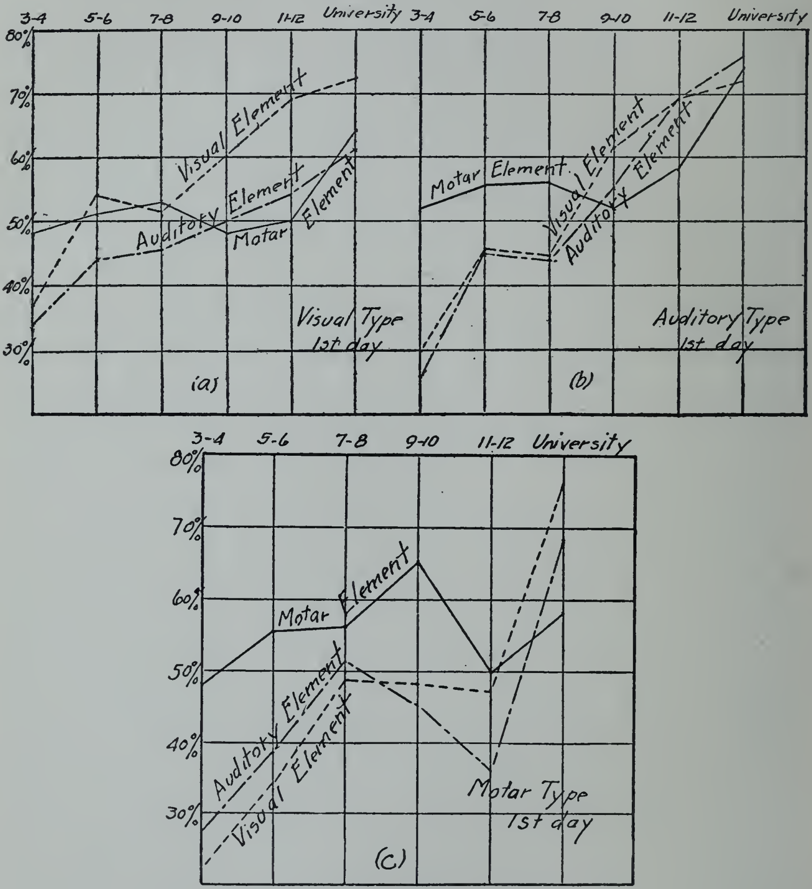

FIG. 7.

tional types, as determined by the first series of tests with nonsense characters and syllables.

By means of these first tests all the subjects studied, as has been explained above, were classed under one or more of the three ideational types. In the test with the story the retentivity of all the subjects of each type for the three different elements of the story was determined. The visually minded subjects were studied for their memory for the visual, auditory and motor elements in the story; the auditory and the motor minded subjects were also considered from a like standpoint. The results of this tabulation are expressed graphically in Figs.

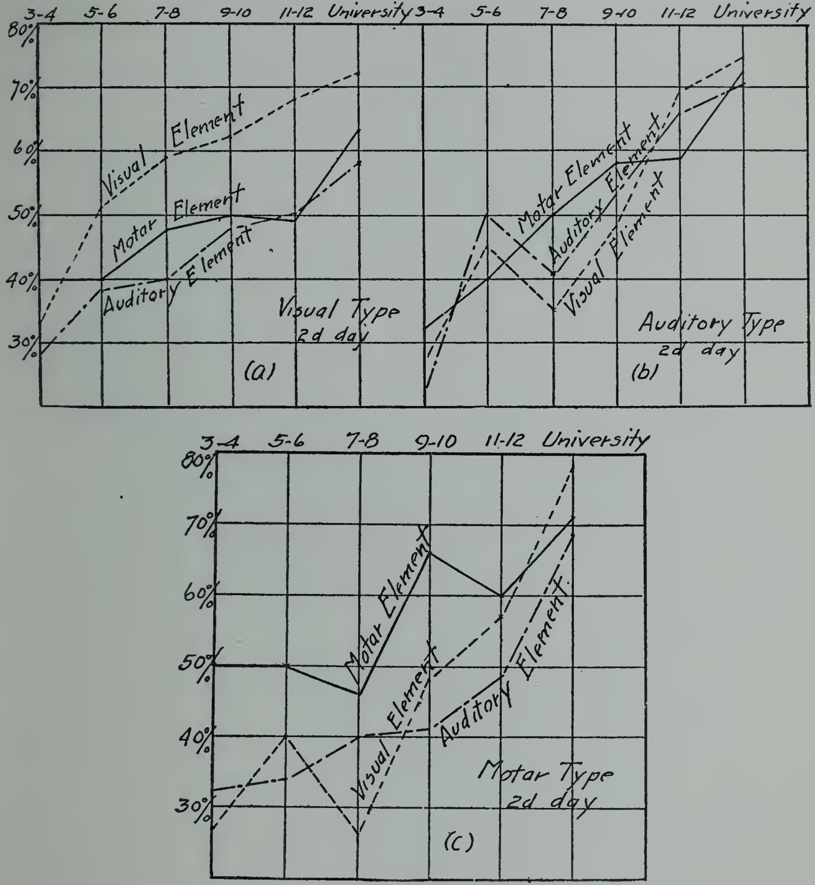

FIG. 8.

7, 8 and 9. Since the classification into visual, auditory and motor types separated each school grade into three groups, and these generally of unequal size, varying as the particular grade under consideration was strongly of one type or of another, some groups were small. For this reason, in the tabulation of the results two grades were combined and the data of the third and the fourth, the fifth and the sixth, and the seventh and the eighth grades were averaged together. The curves represent the results for immediate recall, for recall after an interval of twenty-four hours, and for recall

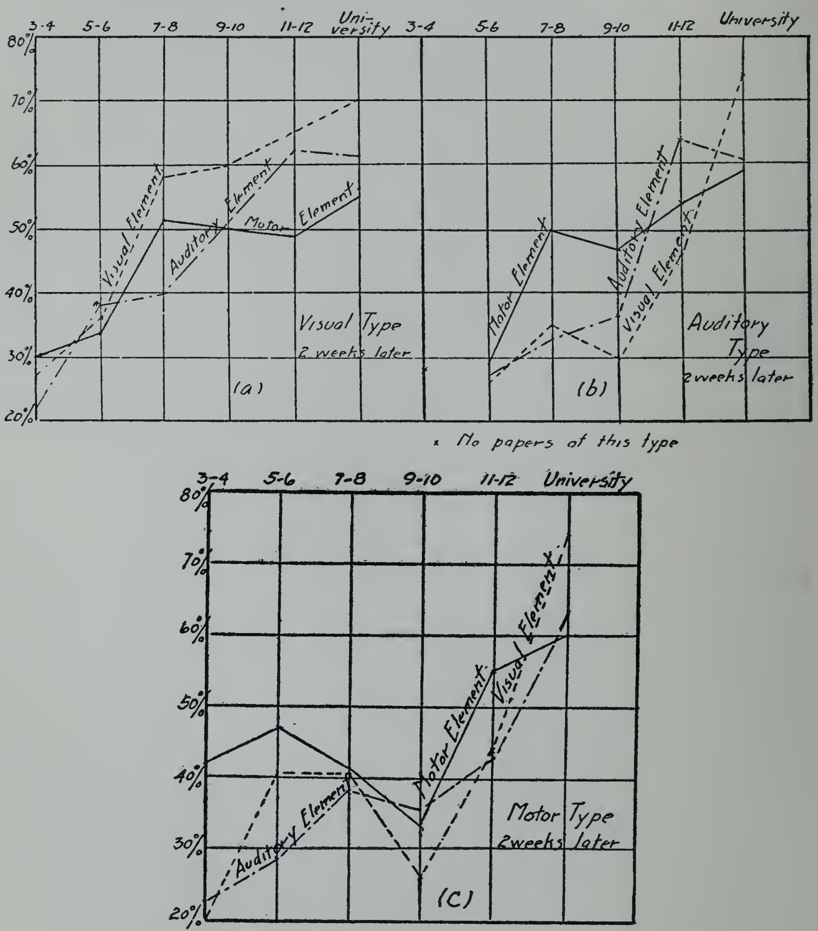

FIG. 9.

after an interval of two weeks. These last named were not as satisfactory as those taken immediately after the reading of the story, or after a lapse of one day, since in some of the grades and in the high school especially, pupils were being examined at the end of the semester's work. This circumstance will explain some of the variations in the results recorded in Fig. 9.

The portions of Figs. 7, 8 and 9 marked (*a*), indicate the manner in which the visually minded subject recalled material appealing to the three sense departments (visual, auditory, motor); the portions marked (*b*) indicate the same for the auditory type, and the portions marked (*c*) for the motor type. Fig. 7 gives the results for immediate recall, Fig. 8 for recall after twenty-four hours and Fig. 9 after two weeks.

It can be seen at a glance that for the early grades the motor elements in the story are better retained than are the visual or auditory. This is true for the three ideational types, both in immediate and in delayed recall. On the other hand, the test with university students shows the superiority of the visual elements, with but one exception for all types, both for immediate and for delayed recall. The superiority of the motor elements of the story in the lower grades is to be explained doubtless in terms of attention and of interest. The narration of what was done by Mr. Jones and the other characters is the most vivid part of the story. It has been pointed out by several investigators that the securing of interest is one of the chief factors in the instruction of young children. This is the all-important thing in the earlier stages of development. By the time the university is reached, however, the mere matter of interest in the story has become much less important, and the tendency towards verbal-visual imagery strongly asserts itself.

Apart from the striking importance of the motor elements of the story in the earlier grades, it will be found in general that the visual elements of the narrative are best retained by the visual type both in immediate and in delayed recall, though the visual elements show their superiority more in delayed than they do in immediate recall, more particularly in the recall after twenty-four hours. The auditory elements are not, however, as well retained by the auditory type as are the motor elements except in the case of the higher grades and the university, and in many instances the visual elements show superiority over the auditory for the auditory type. It can, however, be seen that the auditory type in general retains auditory elements better than does the visual or the motor types. The motor type is at a very distinct advantage in retaining motor material over the visual type except in the university, and in general shows a superiority, though not so pronounced, over the auditory type, particularly for immediate recall and for recall after twenty-four hours. On the other hand both the auditory and the visual type retain motor material as well on the whole as does the motor type when the recall is delayed for two weeks. The curves seem to indicate in general that the visual type possesses a superior retentivity in delayed recall as compared with the motor type, and to an extent as compared with the auditory type. This circumstance tends to confirm in general Meumann's contention that the visual type has greater powers of retentivity than have either the auditory or the motor types. Finally it may be said that in general the visual ideational type is best suited to learn and retain material with a visual content, except in such instances as when material with a motor content appeals so strongly to the interests (particularly of young children) as to gain and hold the attention.[13] The auditory type on the whole and particularly in the lower grades, does not learn and retain material with an auditory content as well as with a motor content, though as a rule visual material is inferior to auditory, particularly in delayed recall. The motor type retains motor material better than it does visual or auditory except in the higher grades. It seems not an unwarranted conclusion (when the element of superior interest in the motor material is removed) that each type of ideation is best suited to learn and retain material appealing to the particular sense department to which this type belongs. An exception to this is to be noted, however, in the higher grades and the university where visual material sems to be better learned and retained by all three types. Here doubtless the auditory and motor types rapidly and with practically no loss translate the material from one sense department to another. This can be readily done since concrete imagery has greatly fallen off and symbolic and verbal thinking has taken its place, a type of thinking to which visual imagery particularly lends itself.

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[13] The factor of interest in movement may explain the results of Colvin and Meyer in the study cited above. Throughout the grades the boys found it their most powerful for of imagery. The girls also showed a highly developed motor imagination. For the boys words with a motor content predominated up to the high school, and for the girls the motor images were in excess of the visual up to the same period. In the grades words denoting both visual and auditory content are markedly lower than the words denoting a motor content, in the case of the boys.

VI. Summary and Conclusions.

The results of the above experiments may be summarized as follows:

1. It seems to be established beyond reasonable doubt that the young child thinks largely in concrete visual imagery, and that while auditory and motor imagery are present to some degree, they play a relatively unimportant rôle in the lower school grades. The child up to ten at least is predominantly a visualizer.

2. Concrete visual imagery, and probably all concrete imagery, tends to fall off in the more advanced grades, its place being taken by verbal imagery. The rise of the latter is probably closely connected with increase in ability to associate and to give meaning to memory material. This ability, however, at first does not compensate for the actual loss in vividness of the concrete imagery, hence there seems to be an actual decline in imagery about the onset of puberty. The subsequent recovery and advance is not due, however, to the recovery of concrete imagery, but to the growth of a power on the part of the adolescent to associate and group into meaningful relations his memory materials. The loss of concrete imagery should be taken into consideration in the schools, and while the pupils should be trained to associate and relate their materials, they should still be educated in such a way as to preserve their ability to think in concrete imagery when such thinking is de-

sirable; and this all the more because it is quite probable that most persons are not by nature predetermined to belong to a particular ideational type. Education has much to do in determining particular types and it is quite possible for a person to think in terms of verbal and symbolic imagery for one purpose and in terms of concrete imagery for another.

3. The importance of motor imagery, both for the hand and for the vocal organs, appears to be much less than has generally been supposed. Doubtless many have confused the interest that children have in movement with an ability or tendency on their part to think in motor terms. Except in pronounced cases where the child is extremely motor in his way of thinking, children seem to depend but little on their motor imagery; indeed, the kinæsthetic sensations from throat and hand may be a hindrance rather than an aid in learning.

4. Auditory imagery shows growth in the period of later childhood. It may, however, be doubted that there is any growth in concrete auditory imagery, which probably is not at any period of development nearly as important as concrete visual imagery. The growth of auditory imagery probably indicates the increase of the verbal type of imagery and its development may show the growing tendency to think in terms of the 'inner-speech.'

5. The rapid falling off of the memory curve as established by Ebbinghaus and confirmed by subsequent experiments does not seem to hold good in the present experiment. Recall after twenty-four hours seems to be as good on the whole as immediate recall, when tested by the method of parts retained, as was the case in the present experiment. This fact is extremely significant in the psychology of learning, and seems to be in harmony with the point of view of Dr. Burnham and the results of several recent investigators.

6. There seems to be a fairly definite relation between the effectiveness of memory in the case of a particular ideational type and the memory material which is most suited to that type. In particular the visual type retains best material with a visual content and the auditory and motor types, to a less degree, material with an auditory or motor content, as the case may be.

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ON THE ANALYSIS OF THE MEMORY CONSCIOUSNESS IN ORTHOGRAPHY.

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COMMUNICATED BY F. KUHLMANN.

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I. NATURE AND AIM OF STUDY.

This study is one of a small class which attempts the investigation of a mental function just as it is found in life. In this respect it differs from most other memory studies whose chief aim has been the investigation of certain factors in or characteristics of memory without direct reference to where or how their influence is to be observed in the concrete ways of daily life activities. As one of the former class, it has to deal with a number and complex relationship of factors whose presence, nature and function it aims to determine. What are the mental processes involved in learning to spell an unfamiliar word, and what is the mechanism of their combined functioning? Some of those with which we are obviously concerned have received separate and special investigation in other memory studies. We know, for example, something about the relative value for memory of visual and auditory methods of presenting verbal material, of the influence of vocalization and other motor processes when combined with visual or auditory perception. But the total mental process induced in the experiments of these investigations has been considerably different from what we find it to be in learning the spelling of a word. Though somewhat the same factors are present in learning, their setting is different and their combined functioning is not the same. For this reason seemingly valid deductions from the results as applied in our present problem become unsafe, and, as we shall see later, are quite fallacious. Our problem might therefore be stated as not that of the separate and isolated investigation of individual memory factors, but as the determination of the complex relationship of all factors entering a mental activity as found in life. This does not preclude their experimental isolation and control. The object of the analysis sought includes the determination of what is the best method in orthography. Assuming this analysis to be made, we may ask what variation and control can be introduced into the total process that will induce a better memory, or result in a saving of time in memorizing.

II. Previous Investigations.

We have for a long time had an *a priori* analysis of the factors that *may* enter the learning and recall of the spelling of a word. It is obvious that in the recall the writing of a word may follow (*a*) the auditory imagery of the letters, or (*b*) the visual imagery of the letters, or (*c*) the incipient vocalization of the letters. It is also obvious that the word may be learned through auditory or through visual perception, and that in either case the learning may be aided by accompanying vocalization, and perhaps also by simultaneously writing it. The history of pedagogical opinion shows that each of these factors in learning has in turn been regarded by different writers as the most important for orthography, and reasons for these different claims have been elaborated with considerable detail. Upon the basis of these opinions the different methods of teaching have been developed, putting different degrees of stress upon the auditory, the visual perception, upon vocalization, and writing.[1] But general observations and deductive methods have clearly not been adequate to determine what the facts are among these mere possibilities.

A. *Visual and Auditory Presentation of Verbal Material.* —In the more recent discussions the results of certain memory studies have been drawn upon to help decide the question. The relative value of visual and auditory presentation of verbal material has received attention from a number of investigators. The general result of their studies has been that either the visual or the auditory presentation may be the better, depending, first, upon the age of the person, and, second, upon whether the material is meaningless or meaningful. For meaningless material, nonsense syllables, visual presentation is the better, irrespective of age.[2] For meaningful material, familiar words, auditory presentation is the better with the younger school children, while visual presentation is the better with the older children and with adults.[3] That auditory presentation should be better than visual with the younger school children, while the reverse holds true with the older children and adults is explained by Kirkpatrick, Hawkins and Pohlmann by the fact that the younger children are less familiar with written than with spoken language, while with age this familiarity with the written language increases, and school methods also change from aural to visual. To this Pohlmann adds that auditory presentation induces better attention than does the visual. As regards the fact that for nonsense syllables visual presentation is better than auditory irrespective of age, Pohlmann notes, first, that the auditory perception of the syllables is difficult; there is often uncertainty as to what the sounds are; second, in auditory presentation, words can be readily translated into visual imagery, while for syllables this is a difficult and slow process.

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[1] A very good review of the pedagogical discussions in Germany on this matter is given by Lay: *Furher durch den Rechtschreib-Unterricht.* Wiesbaden, 1899. See also Meumann, Vorlesungen, Vol. II., Chapter XV.

[2] Pohlmann, *Experimentelle Beiträge zur Lehre von Gedächtnis.* Berlin, 1906.

[3] Pohlmann, Calkins ('A Study of Immediate and Delayed Recall of the Concrete and the Verbal,' PSYCH. REV., Vol. V., 1898), Hawkins ('Experiments on Memory Types,' PSYCH. REV., Vol. IV., 1897) and Kirkpatrick ('An Experimental Study of Memory,' PSYCH. REV., Vol. I., 1894) have obtained both these results. MacDougall ('Recognition and Recall,' *Journ. Philos., Psych., and Sci. Meth.*, Vol. I., 1904) found visual presentation the better with adults. Whitehead ('A Study of Visual and Aural Memory Processes,' PSYCH. REV., Vol. III., 1896) found visual presentation the better for nonsense syllables with adults. Bingham ('Memory,' PSYCH. REV., Vol. I., 1894) found the same for numerals with adults.

B. *The Influence of Motor Processes.*—The influence of motor processes accompanying the visual or auditory perception of the material to be memorized has also been made the object of special investigation. Thus in committing to memory groups of pictures of the deaf-mute alphabet Smith[4] found that the errors in recall were sixteen per cent. less for those series in which the observers were allowed to form the letters with the hand, as compared with series in which this motor factor was excluded. Likewise, others have concluded, from experiments that will be considered in more detail below, that writing words simultaneously with seeing them is a considerable aid in learning them. But this class of motor processes must necessarily be relatively remotely related to learning and memory. The motor processes involved in the vocalization of verbal material show a closer connection. Most adult persons readily observe a considerable tendency to vocalize as they read. Such vocalization is equally readily observable with children. Lay[5] has in a rough way made some observations on the amount of such vocalization on school children of different ages. He found that the degree of vocalization with the individual and the number of children in whom it was readily observable decreased considerably with age. Secor[6] in making a more careful study of the minor movements of the larynx in reading, with four trained observers, concluded that the complete elimination of articulation in reading *is possible*, when special efforts are made to do so. He employed the laryngograph to get a graphical record of the movements of the larynx as well as introspection, and found the two methods about equally good for detecting slight vocalization. Curtis,[7] on the other hand, employing a different form of laryngograph, was able to obtain graphical records of articulatory movements, in mentally reciting a poem or prose, of which the observers were for the most part unaware.

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[4] 'On Muscular Memory,' *Am. Journ. Psych.*, Vol. VII., 1896.

[5] 'Anschauungs- und Gedächtnistypen in Volksschul- und Seminarklassen. Experimentelle Untersuchungen zur Vertiefung des Prinzips der Anschauung.' Leipzig, 1903.

[6] 'Visual Reading. A Study in Mental Imagery,' *Am. Journ. Psych.*, Vol. XI., 1899–1900.

From the results from these different sources it is perhaps safe to say that some tendency to vocalize the words in reading, and in memorizing the verbal material used in memory experiments is almost universally present. In agreement with what would be expected from this, Cohn,[8] Fränkl,[9] Lay[10] and Smith[11] found that when this vocalization is arbitrarily limited or excluded in learning verbal material the amount that can be recalled is correspondingly decreased.

C. *Experimental Studies in Orthography.*—Lay,[12] Fuchs and Haggenmüller,[13] and Itschner[14] have attacked the problem of the analysis of the processes in learning the spelling of a word directly with regard to their relative value. They compared the visual and auditory methods of presentation, attempted to determine the influence of excluding vocalization in different degrees, and the influence of writing the word simultaneously with its visual presentation. As the present study is for the most part concerned with this same general problem, we will consider these experiments in more detail. All these investigators made their experiments on classes of school children in the different grades. Lay and Itschner used nonsense words for material. Fuchs and Haggenmüller used unfamiliar

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[7] 'Automatic Movements of the Larynx,' *Am. Journ. Psych.*, Vol. XI., 1899–1900.

[8] Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses,' *Zeitschr. f. Psychol. u. Phys d. Sin.*, 15 Band, 1897.

[9] *Ueber Vorstellungs-Ellemente und Aufmerksamkeit.* Augsburg, 1905.

[10] *Anschauungs- und Gedächtnistypen.* Leipzig, 1903.

[11] 'On Muscular Memory,' *Am. Journ. Psych.*, Vol. VII., 1896.

[12] *Fürher durch den Rechtschreib-Unterricht.*

[13] 'Studien und Versuche über die Erlernung der Orthographie,' *Schiller's Sammlung von Abhdl. aus dem Gebiete der päd. Psychol.*, II. Bd., 4 H.

[14] 'Lay's Rechtschreibreform,' *Jahrbuch d. Vereins für wissenschaftliche Päd.*, 1900.

German and Latin words. Groups of these were presented to a class in the following different ways:

1. Auditory presentation.
   (*a*) Heard, without vocalization by pupils.
   (*b*) Heard, words pronounced in a whisper.
   (*c*) Heard, words pronounced aloud.
2. Visual presentation.
   (*a*) Seen, without vocalization by pupils.
   (*b*) Seen, words pronounced in a whisper.
   (*c*) Seen, words pronounced aloud.
   (*d*) Seen, letters pronounced aloud.
   (*e*) Seen, copied simultaneously by pupils.

In the auditory presentation the words were read, not spelled out to the pupils. The method of excluding vocalization was that of requesting the pupils to hold the tongue between the teeth while the words were presented. Lay, and Fuchs and Haggenmüller kept the number of repetitions in presenting a group of words the same for a given series, and the presentation time therefore varied, it being necessarily longer for the 2 (*d*) and 2 (*e*) series than for the others. Itschner kept the presentation time constant and varied the number of repetitions accordingly. The directions to the pupils as to vocalizing in 2 (*e*) varied but never called for vocalization of the letters. The following table is derived from their results, the figures being average number of errors in recall per pupil per group of words.

	Lay.	F. and H.	Itschner.	Average.
1a	3.04	1.75	2.93	2.57
1b	2.69	1.90	3.39	2.66
1c	2.25	1.45	2.61	2.10
2a	1.22	.84	1.40	1.15
2b	1.02	.73	1.70	1.15
2c	.95	.76	1.47	.72
2d	1.59	.58	2.10	1.42
2e	.54	.37	1.62	.84

These figures are not in themselves entirely conclusive with reference to all the points that the experiments aimed to decide, since there is considerable disagreement for some of the series. Lay, on the basis of his own results, attributes a great importance to vocalization. In this he is perhaps correct, although Itschner, and Fuchs and Haggenmüller have failed to verify his figures. Itschner concedes that Lay's results are the more reliable because the latter's experiments were more extensive than his own. Many other factors enter to influence the objective results. As these authors have pointed out, on the one hand, vocalization is probably not entirely excluded when the attempt is made to do so, while, on the other hand, whispering and pronunciation aloud is probably a considerable distraction when the experiment is made on a whole class at a time. To this should be added that holding the tongue between the teeth to exclude vocalization also probably causes some distraction to the unpracticed pupil. In the comparison of the relative value of visual and auditory presentation there is an obvious criticism to be made against the conduct of the experiments. It is clear that so far as the words were at all unphonetic the auditory presentation would have to result in the larger number of errors, since the words only and not the letters were read to the pupils. The experiments do not give any results at all on the point in question. A second criticism concerns the failure to keep the presentation time constant for the different series between which comparisons were made. Lay argues that the longer time it took to present the 2 (*e*) series was not the cause of the small number of errors. He made a few special tests on this point. But the results on which he bases this conclusion are not decisive. His tests were made under too varying conditions, and the exceptions to his conclusion too numerous in his results. Itschner's results, for which the presentation time instead of the number of repetitions of a group was kept constant, probably come much nearer showing the correct relation of factors here. It is seen that he got rather more than less errors when the children wrote the words during visual presentation. These three investigators give each a different explanation for the results Lay, and Fuchs and Haggenmüller obtained in series 2 (*e*). Lay attributes the small number of errors for this series to an influence of the writing *per se*. Fuchs and Haggenmüller note that in this series the words are seen twice, once as they are presented, and a second time as they are being written. Itschner explains the results still differently. First, the presentation time, being longer, should reduce the errors. Secondly, when the word is merely seen it readily gives the satisfaction that it is learned, when it is not. But copying the seen word is done from immediate memory. This determines whether the word is learned or not, and gives the opportunity to look a second time at the part that is not learned. We shall be able to demonstrate the validity of this last contention with results of special tests on this matter.

III. Method and Procedure.

Our method and general procedure differed from that of Lay and others in two essential ways. First, only a few and trained observers, four in all, took part in the different series of the experiments. The aim, as was stated above, was first of all a detailed thorough analysis for all the factors entering the total mental process of learning and recalling. Secondly, much stress was put on introspective observation as the chief method of making this analysis. With reference to the number of observers, employed, we recognize the validity of the criticism that in the complexity of any mental function there *may* exist a considerable range and variety of individual differences, which the procedure with few observers may fail to determine. The reply is that any procedure with a larger number of observers which fails to make the detailed analysis for each observer fails likewise in the determination of individual differences of the sort in question, and thus arrives no more at generalizations that might hold true of the average person than does our procedure. Those who have worked on this problem have attempted two different things. First, they have aimed to find the method and conditions of presentation that would give the least errors in recall for the average person. Second, with the average number of errors obtained under the different conditions, they have attempted to analyze the presence and relation of mental processes that produced their figures. The study of the first of these questions obviously requires a large number of observers. For the study of the second question the method of Lay and others is in our opinion quite inadequate, when employed alone. Our procedure is the reverse. It attempts to make the analysis in question mainly through the introspective observations, and employs the figures mainly as an aid to determine the relative influence of the factors found, believing this to be the only safe procedure. The danger of inferring the existence and manner of fuctioning of a mental process from objective results in so complex a function as memory has been often enough demonstrated. In this case we have the inference of what is a possibility; in introspection we have the observation of what is a fact. We feel assured that if Lay and his associates had employed direct observation with trained observers they would have explained their large number of errors in their auditory presentation of words, and the small number of errors when in addition to seeing the words the pupils also wrote them out, differently, and they would also have been able to point out the significance of the large number of errors when the pupils were required to vocalize the individual letters during presentation of the word.

With the four observers mentioned, five different series of experiments were undertaken, two observers taking part in each series, not counting preliminaries. The series were taken successively, their nature being determined by what seemed important from the progress of the study. In all cases unfamiliar and difficult words were used, uniformity being obtained, first, by trying to make them so in choosing the individual words, and, second, by collecting a large number and then making up the different ten word groups used for a series by selecting by chance. The different series were as follows: (1) Comparison of visual and auditory presentation. (*a*) Successive visual presentation of the individual letters of a word at the rate of two letters per second and two seconds intervening before the beginning of the next word of a group of ten. The successive exposure of the letters was made with a kymograph. (*b*) Successive auditory presentation of the individual letters of a word at the same rate as in the visual presentation, and with all other conditions the same. In this the words were spelled out to the observer, the experimenter following a metronome, inaudible to the observer. The observers, who will be designated by the letters *A* to *D*, were *A* and *B*. Ten groups of words of ten words each for each observer for the visual and for the auditory presentation were used. (2) The influence of vocalization by the observer of the letters or of the syllables. In this the letters of a word were presented simultaneously, visually. The words of a group were presented successively. A word was exposed for seven seconds, with one second intervening before the next word of a group was given. (*a*) The observer tried to rule out all vocalization, the tongue being held between the teeth. (*b*) The observer avoided vocalization of the individual letters of a word, but pronounced the syllables silently. (*c*) The observer vocalized the individual letters but not the syllables. *C* and *D* observed in this series. Six groups of ten words for each observer for each of (*a*), (*b*) and (*c*) were used. (3) The influence in visual presentation of syllabification and the use of diacritical marks of pronunciation. In this a group of words was presented simultaneously for a total time equal to the total time spent in the presentation of a group in '2' and in all the following series. (*a*) There was no division into syllables and no diacritical marks of pronunciation. (*b*) The word was divided into syllables and the diacritical marks were added. *C* and *D* observed. Ten groups of words for each observer for each of (*a*) and (*b*) were used. (4) Successive exposure of the words of a group compared with simultaneous exposure. For this comparison no separate series was taken. The results for '3.(*a*),' and '5.(*c*)' give this comparison. (5) The influence in visual presentation of a few seconds interval allowed for the immediate recall of an exposed word before exposure of the next word of a group. In this the total time spent on a word remained constant at eight seconds, but the relation between the time given for exposure and the time given for immediate recall varied as follows: (*a*) Three seconds exposure and five seconds for immediate recall. (*b*) Five seconds exposure and three seconds for immediate recall. (*c*) Seven seconds exposure and one second for immediate recall. *C* and *D* observed. Ten groups of words for each observer for each of (*a*), (*b*) and (*c*) were used.

In all cases the words were given in groups of tens. In all cases, except the simultaneous presentation of the ten words of a group, a group was presented three times, giving 240 seconds as the total time for learning ten words. For presenting the words successively and regulating the relation of the exposure time for a word and the time allowed for immediate recall a special apparatus was used. One part consisted essentially of a large camera on the ground glass of which the observer saw the image of the typewritten word, this giving a means of regulating the size of the letters and the distance from the eyes independently. A second part consisted of a fall apparatus, the drop-board of which carried the sheet of typewritten words in columns of ten. A third part included a metronome with electrical connections driving a special contact apparatus which made and broke a circuit at the relation of intervals stated. In the circuit of the latter was placed an electro-magnet whose attached lever dropped and held the drop-board of the fall apparatus in the corresponding intervals. Immediately after a group was learned the experimenter pronounced the words to the observer for recall, repeating if necessary. The observer then took as much time as he wanted to spell the word, the experimenter and not the observer writing down the spelling given. In the first two series only an immediate recall was taken. In the third series the recall was repeated four days after the immediate. In the fifth four recalls were taken, the second being four days after the immediate and the third and fourth eight and sixteen days, respectively, after the last recall. The results show, however, that the number of errors in recall did not increase after the second recall because the words were by this time entirely forgotten. For these last two recalls in the fifth series the results will, therefore, not be considered. For all the series it was attempted to get a thorough introspective analysis of the manner of learning the words, and of the process of recall.

IV. ANALYSIS OF RESULTS.

A. *The Recall and Recognition Processes.*

In pronouncing the word to the observer for spelling, it was found that the processes that entered into the recognition of the word and the recall of its spelling, with one exception, did not vary in any noticeable degree with the manner of learning the word or with the method of presentation. The recall and recognition processes were the same in all the series taken, including auditory presentation. This was not the case with the factors entering the learning of a group of words. We may, therefore, consider recall and recognition first, leaving the analysis of the factors in learning for a separate treatment for each special series.

1. *Recognition of the Word as a Whole.*—It will be important to bear in mind from the outset that we never have in this experiment the ordinary case of recognition; a stimulus as given in perception is never repeated for recognition. In auditory presentation the observer heard only letters. In visual presentation he saw only letters. In both cases he was given the *sound* of the *word* when it was pronounced to him for spelling. If, therefore, he recognized the word when pronounced to him it could not be recognition of what he had in perception. The recognition must in this case be through the imagery or motor processes that were associated with the perception in learning and which was aroused again by the sound of the word. It thus happens that the manner of recognizing gives a secondary means of determining the prominence and function of the processes in learning.

The observations require a distinction to be made between the recognition of a word as a whole and the recognition of the correctness of its spelling as recalled by the observer. Quite often the two were entirely separate processes. Of the recognition of the word as a whole three forms were observed. (*a*) The word was at once recognized on pronunciation to the observer. No secondary processes were present to mediate recognition; it was direct through the sound. This was the usual form of recognition, by far the majority of cases belonging to this class. (*b*) The word was recognized through its visual image that the unrecognized sound aroused. (*c*) No definite recognition of the word entered, but its spelling as finally decided upon was in some degree accepted, mostly through other than memory factors. In a rough way these three forms of recognition were correlated with different degrees of familiarity of the words, the most familiar or best learned words belonging to the first class. The incipient or actual pronunciation of the word by the observer was sometimes regarded by *D* as adding something to the recognitive familiarity in the case of an easy word. But for *D* the vocalization and the auditory image of the sound as he would himself pronounce the word constituted an all but inseparable complex. It is, therefore, possible that even in this case the vocalization was an aid only indirectly, through the auditory image associated with it. For *C* this auditory image seems to have been less prominent, and vocalization was never mentioned as an aid to recognition. In learning, *C* pronounced the syllables, as did *D*.

On the whole, it seems valid to infer from this that when the word is recognized through its sound, which has never been heard at all before, the auditory image must have been an important, although not necessarily a consciously prominent factor in learning. The vocal process becomes important then to the degree in which it is required for the arousal of this auditory image.

2. *The Processes in Recall.*—The recall of the spelling of a word and the determination of which were the correct letters was usually a very mixed process, and one in which the syllable much more than the letter stood out as the unit. We may distinguish four forms of recall, but it must then be borne in mind that in any given word the different syllables might belong to two or all four forms. (*a*) The word was spelled through immediately on hearing it, with little or no definite visual imagery of the letters. The observer recognized the correctness of the spelling from his vocal-auditory process. (*b*) More or less definite visual imagery of the letters entered, either at once on hearing the word or accompanied the vocalization in spelling. But the attention was not directed to this

visual imagery, which came up of itself and did not seem to be necessary for spelling or recognition. (*c*) The vocal-auditory process was insufficient for recall or recognition and was not noticeably an aid. The recall was through definite visual imagery of the letters, which either came up immediately and correctly, or different letters were voluntarily visualized and those chosen which looked correct. (*d*) The sound of the word was definitely and consciously taken as a guide in constructing a spelling, with definite visual imagery of the letters as in the last case, which was then made more or less phonetic. There was little or no recognition, in the technical sense, of the correctness of the letters chosen, which were accepted more or less as correct on the basis of non-memory factors.

These four forms of recall were rather closely correlated with the different degrees in which the words had been learned and their familiarity, the easiest belonging to the first, the most difficult to the last. They correspond very well to the general rule that in any given complex mental process whose end factor is a motor process, the subsidiary imagery which is at first an aid in learning is gradually eliminated until only the beginning and the end factors remain, in this case the sound of the word as pronounced to the observer and his vocalization of the letters in spelling. In agreement with this and the degree of memory induced in this expirement, but very few cases belonged to the first form of recall even for the recall immediately after the group had been presented. A few more belonged to the second class, but over three fourths in immediate recall belonged to the third. The last was particularly prominent in the delayed recalls and for the specially difficult words. This range of variety of modes of recall was found by observer *D*. *C* never reported the first form, with whom the visual imagery was in general more prominent and vocalization less so. For the typical total memory process we have left, therefore, one in which the word is recognized directly through the sound as pronounced to the observer, and the recall of its spelling takes place in terms of visual imagery of the letters. In this the complete absence of all auditory imagery of sounds as given in auditory presentation deserves special attention. No auditory imagery of heard letters ever played a part in recall. On the other hand, another class of auditory imagery seems to have been more important for recall than its prominence in consciousness as determined by the introspections would lead one to infer. This is the auditory image of the syllable or word as the observer hears himself incipiently pronounce it. This is, therefore, imagery that is necessarily very closely associated with the vocal process, and is entirely independent of the method of presentation. It may come in quite as readily with the observer's pronunciation of the seen letters as with his pronunciation of the heard letters. Such imagery was only occasionally present in recall, coming to the foreground when the experimenter's prounuciation of the word was considerably different from what the observer had used in learning. In such cases the observer's statement was frequently to the effect that the word as he heard it pronounced suggested his own pronunciation, which in turn at once gave the visual imagery of the letters. We have to deal here with a factor, the closely associated vocal-auditory process, which apparently may play a great rôle in learning but which for the most part does not enter as a means of recall. But since in recall it is the sound of the word that is to suggest the visual imagery of the letters, it should follow that whatever helps to fix the association between this sound and the imagery of the letters will be an especially effective aid for memory even though the word is quite unphonetic in its spelling.

B. *Comparison of Visual and Auditory Presentation.*

1. *Factors in the Learning Process as Affected by the Method of Presentation.*—It will be remembered that in this first series, comparing the visual and auditory methods of presentation, all the other objective conditions for the two methods were identical, the observer seeing the letters of a word successessively at the rate of two a second in the one case, and hearing them at this rate in the other case. Only the results of observers *A* and *B* figure in the table below. But in the preliminaries for the series, taken by *C* and *D*, the latter made out the introspective analysis of the factors in the learning as well as

did *A* and *B* in the regular series. What is given in this analysis holds true of all observers. In fact, no individual differences of importance for the present comparison were determined.

In the visual presentation the main effort of the observer was directed to getting the letters combined into syllables. Unless he succeeded in this the whole word was instantly forgotten, before the next word appeared, about as completely as would have been the case if nonsense syllables had been substituted for the letters and presented at this rate. This combining was done by visualizing the several letters of a syllable simultaneously, which necessitated holding in mind in terms of visual imagery the letters that were just passed until a recognized syllable was completed in presentation, at which point the whole syllable usually flashed out definitely and clearly. With this clear visual imagery of the syllable the latter was usually pronounced, this vocalizing being more prominent for *B* and *D* than for *A* and *C*. *D* noted that there was a considerable tendency to pronounce the individual letters as they were seen, but that such pronunciation interfered with combining the letters visually into syllables, there not being time for both processes. *A* and *C* never vocalized the letters.

In the auditory presentation the process of learning was not essentialy different in the nature and function of the factors that entered. The main effort was again directed to getting the letters combined into syllables, and for the same reason. The method of combining them was again that of visualizing them simultaneously. The individual letters were at once visualized on hearing them, and the observer worked no more with auditory imagery in auditory presentation than he did in visual presentation. The tendency to pronounce the visualized syllable was on the whole somewhat less than for visual presentation. But this seems to have been the result of the slowness of the visualizing process and the short time the conditions of the experiment allowed for it; the visualizing took most of the attention and time of the observer.

2. *The Errors in Recall.*—It has already been stated that the manner of learning a group of words and the method of

its presentation did not noticeably affect the process in recall. It must be added here, however, that the successive presentation of the letters in both cases of this series was a poor method, in that it hindered syllabification, which is seen already to be an important process. From this it resulted that about half of the words were not recognized at all by the observers when pronounced to them immediately after a group had been presented. We may turn at once to the objective results for this series. Table I. gives the average number of errors made in immediate recall per group of ten words for observers *A* and *B*.

TABLE I.

	(*a*) Visual.	(*b*) Auditory.
A	19.3	24.3
B	12.0	16.0
Average	15.7	19.7

This difference in the number of errors in recall in favor of the visual presentation is undoubtedly not as large as it should be to give a correct idea of the relative value of the two methods, inasmuch as only a low degree of memory was induced in either case. The observers' statement that only about half the words were recognized at all when pronounced to them is reinforced by the further fact that the number of errors is, roughly speaking, about four times as great as for the immediate recall of later series. It is evident, therefore, that the difference between the two methods is at least to some extent reduced because the results are in part reduced to a common level by the words being so poorly learned in either case. The total presentation time for a word was approximately the same in this series as in the others. We have, therefore, two things to account for. First, the large number of errors in this series for both methods of presentation. Second, the difference in the number of errors for the visual and auditory presentation. The answers to both are clearly given in the introspective observations. The letters of a word must be presented simultaneously to facilitate syllabification, if the word is to be learned readily, for syllabification is essential. Secondly, when they are presented successively the auditory presentation is the poorer because the observer finds it impossible to work with auditory imagery of the heard letters as he does with visual imagery of the seen letters. Substituting visual imagery for the heard letters takes a good portion of the time and also of the attention from other essential processes, the combining into syllables and pronunciation. In addition to this, since the recall is in terms of visual imagery in both cases, we have the recall of what was given in actual perception for visual presentation, and the recall of what was present only in the form of imagery for auditory presentation. Since in auditory presentation the letters must necessarily be given successively, there are these several reasons why it should be a poorer method than the simultaneous visual presentation of the letters.

C. *The Influence of the Observers' Vocalization of the Letters or of the Syllables.*

In this second series the observer learned the group of words in successive visual presentation in the one case without any pronunciation of the letters or syllables, so far as possible, in the second case he pronounced the syllables and not the letters, and in the third case he pronounced the letters and not the syllables. As is seen, this series was intended to determine the influence of vocalization, and to discriminate between the influence of vocalization of letters and of syllables. The results of the first series suggests that these two kinds of vocalization may differ considerably in their effect on learning.

1. *The Observers' Direct Observations.*—In this and in the following series we have to take individual differences into account. In learning a group of words *C* had no difficulty in excluding the vocalization entirely, or in vocalizing syllables only or letters only. *D* found it quite impossible to entirely exclude the vocalization. The manner of learning for *C* and *D*, therefore, varied considerably. Observer *D* noted that when, or in so far as, he succeeded in eliminating all vocalization in learning, the words would not even be recognized as words as he looked at them, and that the attempt to keep out the vocalization was a great distraction. What usually occurred in

place of this was a very incipient and incomplete vocalization of the syllables. A special feature of this procedure was the coming into prominence of the auditory imagery of the syllables; *D* heard himself pronounce the words in imagination, and this auditory image would ring out the clearer the more the vocal part of the vocal-auditory process was voluntarily subdued.[15] Breaking up the word into syllables through a purely visual process was impossible for him; syllables had no meaning or existence even, except as accompanied by a vocal-auditory process. The vocalization of the letters only tended also to prevent the syllabification and non-recognition of the word. In this, syllabification took place in part by the auditory image of the syllable flashing in with the pronunciation of its last letters. For several groups of words taken last in this series, syllabification was aided by pronouncing the letters of the syllables in rhythms, with long pauses between syllables. *D's* natural manner of learning a word included the pronunciation of the syllable only in case of easy syllables, and the pronunciation of the individual letters in addition in the case of difficult syllables. On the whole, he regarded vocalization of the syllables only as the best method, but vocalization of the letters of hard syllables as necessary, while the attempt to exclude, so far as possible, all vocalization was both extremely difficult and seemed poor method.

Since *C* had no difficulty in excluding the vocal process, the manner of learning was for this observer quite in accordance with what was demanded by the experiment in this series. One further point of difference, however, which was a direct result of excluding the vocal process, is to be noted for *C*. Since throughout this series a word was presented for seven seconds with only one second interval following before the presentation of the next word, there was considerable opportunity for trial recall during the presentation time. This is a natural procedure and it requires a special effort to exclude it when conditions for its possibility are given. We shall give this a special consideration later. In the present case, trial recall during the presentation of a word entered to some extent when all vocalization was excluded, but never when the letters were pronounced while learning. Since pronunciation of the individual letters was for *C* the unnatural method of learning which required special effort to follow, and since it is at the same time a much slower process than merely noting the letters visually, we have this double reason why trial recall should enter in the one case and not in the other. Pronunciation of the syllables only seemed to *C* the easiest and most natural of the three ways of learning, and pronunciation of the letters by far the poorest and most unnatural way.

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[15] This seems to be in harmony with a result obtained by Secor, who found that in the attempt to exclude articulation in reading the auditory element was much more persistent than the vocal. *Am. Journ. Psych.*, Vol. XI., 1899–1900.

As regards the readiness of recall and recognition of a word when pronounced to the observer, *C* noted some differences for the different ways of learning that are of special significance. When the syllables were vocalized in learning the word was at once and readily recognized when heard. When the letters only were vocalized, or when there was no vocalization in learning, recognition of the word was much less certain. Further, the heard word did not suggest the visual imagery of the letters readily, and, in the case when letters only had been vocalized in learning, the visualized letters looked indifferent as to correctness. This reflects in some degree the manner of functioning of the vocalization of syllables.

2. *The Errors in Recall.*—Table II. gives the average number of errors per group of ten words, as before, for the three ways of learning.

TABLE II.

	C.	*D.*
(*a*) No pronunciation	5.7	13.7
(*b*) Syllables only pronounced	4.3	8.0
(*c*) Letters only pronounced	14.7	7.7

It appears from these figures that only the second method of learning had the same influence for the two observers, while for the first and third methods the results for the two observers are in complete disagreement. These individual differences, however, are the differences that were to be expected from the analysis of the learning process already given, and are in accord with the results of the following series. The ease with which *C* excluded the vocal process in learning, and the difficulty *D* had in attempting to do so may be taken in part as a measure of its relative prominence and value for the two observers, but perhaps only in part. Thus we find that the attempt to exclude it entirely increased the number of errors in recall some for *C*, but nearly doubled it for *D*. As regards the effect of the vocalization of syllables and the vocalization of letters, we must recall here that according to the direct observations of *D* one seemed the better method for easy syllables, the other the better for difficult syllables, and either a much better method than the first. The relative number of errors in recall for *D* verify his introspective analysis. The large number of errors for *C* when letters only were vocalized in learning indicates that this vocalization has been a positive hindrance either to the purely visual method of learning or to the combining of letters into syllables through the pronunciation of the latter, or to both. Since pronunciation of the syllables only is the best of the three methods for *C*, it is to be inferred that it is due in part to the latter. Results of the next series, however, will show that interference with a purely visual process of learning was for *C* especially prejudicial to the best results.

D. *The Influence of Syllabification and the Use of Diacritical Marks of Pronunciation.*

With the fact established that the mode of recall is, as a rule, through visual imagery of the letters, the problem for the learner becomes that of getting as close an association as possible between the sound of the word as it will be given to him for spelling and the visual imagery, because it is this sound that is to suggest the letters either directly or through his vocalization with which he may repeat the heard word. Since in learning the observer does not hear the word, but only the letters, or sees only the letters, he must supply this vocal-auditory process himself. In the preceding series we have attempted to regulate the vocal process during the learning; over the auditory imagery that may be associated with this we have but little direct control. But requiring the observer to pronounce the syllables in learning insures his getting this association between the sound of the word and the visual imagery of the letters only so far as he pronounces the word correctly. The present series aimed to determine what the effect would be if aids to this correct pronunciation were introduced. This might be done by pronouncing the words to the observer simultaneously with their visual presentation, or the written word as presented might be divided into syllables and the diacritical marks of pronunciation added. After a few preliminary trials the latter was chosen as probably the better method.[16] In this third series the ten words of a group were presented simultaneously, in the one case the pronunciation being indicated as stated, in the other no such indication being given.

1. *The Observers' Direct Observation on the Learning.*—It was found at once in the introspections that the spacing of the syllables and the presence of the diacritical marks would have two different effects, opposed to each other in their manner of affecting the number of errors made in recall. Observer *C* noted that the spacing and the marks introduced a confusion in the visual process of learning. So much was this the case that in the trial recalls during the learning the words were purposely visualized without the spaces and the marks. This resulted in it taking about three times as long to go over the group of words for the first time. *D* noted the same confusion entering to some extent, but for him this seems to have disappeared later. On the other hand *D* found that he could go over the group of words in less time when the spacing and marks were added because of the reduction of the time required for breaking up a word into syllables and determining the pronunciation.

2. *The Errors in Recall.*—The next table, giving the number of errors in the same way as before, brings out the fact

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[16] As will be seen below, the results of this series did not prove the correctness of this first impression. It is quite likely that for some observers the former method would be the better. Time did not permit taking a special series to determine this point.

that both of these influences of the spacing and marks have been considerable, one for one observer, the other for the other.

TABLE III.

	(*a*) Pronunciation not Indicated. Immediate Recall,	4d. Recall.	(*b*) Pronunciation Indicated. Immediate Recall,	4d. Recall.
C	2.9	18.0[17]	5.8	20.0[18]
D	4.1	21.9	2.3	14.5
Average	3.5	20.0	4.1	17.3

Roughly speaking, the indication of the pronunciation has doubled the number of errors for *C*, and reduced them a half for *D*. This individual difference is in accordance with that found in the second series, both showing that non-interference with the visual process in learning is of the first importance for *C*, while non-interference with the correct vocalization of syllables is of equal importance for *D*. We are not to conclude from this, of course, that the indication of the pronunciation was in itself not an aid for *C*. For in the second series we have already seen that pronunciation of the syllables gives the least errors, and an aid to correct pronunciation, if other conditions remained the same, should reduce the errors further. On the other hand, we found that separating the syllables and adding the marks was for *D* also some hindrance in learning.

E. *Comparison of Successive and Simultaneous Exposure.*

For the successive exposure that will be considered in the present comparison a word was exposed for seven seconds with an interval of one second following before exposure of the next word. The results of this will be compared with those of the preceding series for which no pronunciation was indicated. We are concerned now with factors that do not belong exclusively to learning and remembering the spelling of words. But their study was undertaken because they seemed of special significance for the present problem.

1. *The Observers' Direct Observations on the Learning.* (*a*) *Successive Exposure.*—Some matters to be taken into account at this point have already been stated in connection with

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[17] Average for seven groups of words only.

[18] Average for nine groups only.

the previous series. We may recall that the usual procedure included an attempt to break up the word into syllables and to fix on the pronunciation of the word, and that for observer *D* the letters also of the difficult syllables were vocalized. The procedure demanded that all of the seven seconds should be used in looking at the word and that all trial recall should be excluded. But seven seconds was often longer than necessary to go over the word several times, and the tendency to try to visualize especially the difficult syllables to determine whether the word was learned was constantly present. Trial recall, therefore, entered to some extent, and is at once some index of its importance.

(*b*) *Simultaneous Exposure.*—For the individual word the procedure in learning was quite the same as in the successive exposure, except in the amount of trial recall that entered. In this case the observers were allowed to do as they chose, and trial recall for a word until it could be spelled in immediate memory was the rule. The simultaneous exposure resulted also in a distribution of the time for the ten words of a group according to the ease with which they could be learned. A third difference consisted in a lack of uniformity of attention and effort in the present case. Successive exposure with always a definite and limited time allowed for each word was a considerable aid in keeping the attention constantly near its maximum. Simultaneous exposure with the opportunity to return to a word at any time and no definite knowledge of how much of the total time allowed was left favored a lowering of effort.

2. *The Errors in Recall.*—

TABLE IV.

	(*a*) Successive Exposure.		(*b*) Simultaneous Exposure.	
	Im. Recall.	4d. Recall.	Im. Recall.	4d. Recall.
C	5.4	16.8	2.9	18.0
D	6.1	11.4	4.1	21.9
Average	5.8	14.1	3.5	20.0

The comparison of these figures brings out two things. First, for the recall immediately after a group had been learned the number of errors is much smaller for simultaneous exposure than for the successive. Second, for the second recall four days after learning the reverse holds true. Considering the differences in the manner of learning in the two cases, we may offer the following interpretation. In the simultaneous exposure each word is learned only to the degree required for successful recall the moment after it is seen. When all the words have been learned in this manner the effort lags because of the feeling of assurance that all can be recalled. The use of trial recall makes this possible, and is also the means of distributing the time correctly for the ten words. This results in few errors in immediate recall even for the difficult words, because more time has been put on these. It results also in many errors in the delayed recall because of the many words that have been only barely learned and on which more effort might have been spent. In the successive exposure the constant maximum effort and the equal amount of time spent on all the words result in many words being thoroughly learned, reducing the errors for these in the delayed recall. The relative absence of trial recall and the impossibility of giving more time to the difficult words than to the easy ones result, on the other hand, in many words not being learned at all and increases the number of errors for the immediate recall.

F. *The Influence of a Few Seconds for Immediate Recall Following the Exposure of Each Word.*

As was stated above, in the fifth series the time allowed for each word for one presentation of a group remained constant at eight seconds. But in one case each word of a group was exposed for three seconds with five seconds following for immediate recall, in a second case the exposure time was five seconds with three seconds following, and in a third it was seven seconds with one second following. These relations of exposure time and interval following for immediate recall were chosen after some preliminary observations as probably including the optimum. The observers were instructed to use the exposure time exclusively for the perception of the word, and the interval following exclusively for immediate recall. We have seen the influence of trial recall, which gives the observer an opportunity to find the difficult points and to know when he has overcome them. This includes the immediate recall with which we are now concerned. The object of this series was to determine whether it was not better to spend a part of the time on such recall than to put it all on perception, even though the observer could not return to the word when he found that he could not yet recall it, nor put more time on the difficult words and less on the easy ones.

1. *Direct Observations.*—The three seconds exposure was often found to be too short to complete the processes for a difficult word, namely, breaking it up into syllables, pronouncing it, and giving special attention to the difficult syllables. This was especially the case with observer *D*, who included vocalization of the individual letters of difficult syllables. The seven seconds exposure was usually more than necessary, and trial recall tended to crowd into the exposure time. The immediate recall in the interval following the exposure demanded, on the whole, more time than the perceptive processes, yet about the same was found with reference to the five, three and one second, respectively, allowed for this in the three cases. The five seconds were sometimes more than necessary, and the one second was never enough to do more than to pronounce the word and get a rough incomplete visual image of it. A difference in the uniformity of attention for these different relations of exposure time and time for immediate recall was observed. The attention was naturally kept up best when the time allowed for perception and for immediate recall was in each case just sufficient and not more than necessary to complete the processes entering into each. More time than was necessary to complete them was often wasted.

2. *The Errors in Recall.*—The 3–5, 5–3 and 7–1 in the following table designate the relation of exposure time for a word and interval following in the three cases.

TABLE V.

	a. 3–5.		*b.* 5–3.		*c.* 7–1.	
	Im. R.	4d. R.	Im. R.	4d. R.	Im. R.	4d. R.
C	3.4	15.6	4.7	20.3	5.4	16.8
D	4.8	9.3	5.0	8.0	6.1	11.4
Average	4.1	12.5	4.9	14.2	5.8	14.1

Of these figures those for the recall immediately after a group had been learned interest us chiefly. For the delayed recall they show no conclusive differences as regards the effect of the relation of exposure time and interval following. This is probably due to the fact that the differences are in the first place not so great for the immediate recall as in the comparisons of the tables above and are reduced still more as the words are more entirely forgotten. For the recall immediately after a group had been learned we see that the number of errors is least for the 3–5, and greatest for the 7–1 relation. We may conclude, therefore, that it has been of special value for the memory of a word to convert the perceptive process into terms of imagery instantly after the word was seen; of more value than if the time for it had been spent on repeating the perceptive process. That this should be true still when more than half the time is given to this imagery indicates how very essential it must be.

V. Summary and Conclusions.

One of the things determined will be seen to be of more importance than its separate consideration above indicated. This is the fact that, irrespective of the method of presentation and the manner of learning, the *typical* mode of recall for all observers was through the visual imagery of the letters. This being the case, the problem for the observer becomes, psychologically stated, that of getting as close an association as possible between the sound of the syllables and the visual imagery of the letters.[19] The sound of the word as given the observer for the recall of its spelling must suggest the visual imagery of the letters, whose individual sounds may not be at all indicated in the pronounced word. Secondly, since the individual letters must be presented in learning, there are two things in the first term of this association between the sound of the syllables and the visual imagery of the letters that may be aided in learning: we may facilitate getting the sound accurately, and we may facilitate the auditory or visual perception of the letters.

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[19] We are dealing, of course, only with words in so far as they are unphonetic, since words that are spelled strictly phonetically present no problem for memory at all.

The visual mode of recall also enters as one factor in determining the relative value of the visual and auditory method of presentation. It seems to have been tacitly assumed by some writers that the recall might be in terms of auditory imagery whose arousal might be the easier when the perception has been auditory instead of visual. Our results show, first, that visual imagery is invariably substituted at once for the heard letters, and, second, that the heard letters are never recalled in terms of auditory imagery. This fact alone makes the auditory method of presentation poorer than the successive visual presentation of the letters. The other results, however, on this comparison of the two methods of presentation are of more importance. We have found that the observer's combining the letters at once into syllables as they are presented is of prime significance, and that the successive presentation of the individual letters prevents this. The necessity of giving the letters successively in auditory presentation, therefore, rules out this method as a competitor with the simultaneous visual presentation of the letters.

We see from this that learning the spelling of a word is a quite different process from memorizing a group of words or nonsense syllables, and that the fact that under certain conditoins auditory presentation for the latter is better than the visual does not permit of any conclusion to be drawn with reference to the relative value of these two methods of presentation in learning the spelling of a word.

Lay and his associates have concluded that vocalization is a considerable aid in learning, although Itschner's figures alone do not bear out this statement. We have found that its value depends, first, upon whether it is vocalization of the syllables or of the individual letters, and secondly, upon the relative predominance of the visual processes of the individual observer, and, thirdly, that an auditory imagery connected with the observer's incipient or actual vocalization of the syllable plays a part, and that it is this vocal-auditory process in learning with which the visual imagery of the letters becomes associated. Thus the observer's vocalization of the syllables is always an aid, but the vocalization of the individual letters may be a hindrance. Other points in the analysis show how the latter may take place. The vocalization of the letters is, in the first place, a slow process and prevents going over a word as often as when it is omitted. In the second place, it may be a hindrance to combining the letters into syllables, especially for the observer who is very predominantly visual. We may offer this as an explanation of Lay's results also, who found that when the children were required to vocalize the letters more errors were made in recall than when such vocalization was not required. It is to be noted further that we have here a process that is an aid in learning, but is not as a rule present as an aid in recall. The vocalization of the syllables probably aids the learning in two different ways. First, in connection with the auditory imagery it may arouse, it determines a correct or fixed pronunciation. Secondly, it is probably a guide to the visual perception, causing especially the grouping of the letters into syllables. It is thus a process that facilitates both factors for the first term of the association mentioned.

We have attempted to determine the influence of a special aid to the vocal-auditory process in learning, by indicating the pronunciation of the word. The results of this series show better than those of the others that the best method of presentation and of learning is a matter of getting the right combination for the individual person, of the several processes that may enter. For the observer for whom the vocal-auditory process was more prominent this aid to the vocal-auditory process reduced the errors in recall about a half. For the other observer, whose method of learning was seen throughout to be more purely visual, the effect of this aid was so slight, and that of the hindrance to the visual process introduced with the spacing of the syllables and the diacritical marks, so great that the latter outweighed the former to the extent of doubling the number of errors in recall. This brings to our attention the importance of presenting the word in such a way as to facilitate its visual perception as much as possible. Several other instances have been mentioned in this study in which a factor that retarded the rate at which the word could be gone over in perception was found to be a great hindrance. In this connection we must mention the results of studies on the psychology of reading, which show that the rate of reading varies with even a small difference in the size of the letters and style of type.[20]

Finally, turning to other questions than those concerned with the processes present in learning and recalling, their combined functioning and relative value, we found two other matters of importance. First, the use of trial recall in learning, giving the observer the opportunity to distribute his time for the different words accordingly as they are easy or difficult, and to determine when a word is learned. The simultaneous exposure of all the words of a group, making trial recall possible, reduced the errors in recall immediately after a group had been presented from 5.8 in successive exposure to 3.5. But this favorable effect for the simultaneous exposure is lost in the delayed recall, for which the errors increased from 14.1 in successive exposure to 20.0, this being probably due to the complex relationship of factors described above. Secondly, the results of the last series show that a good part of the favorable effect of the trial recall in the simultaneous exposure is due merely to converting the perceptive process immediately into imagery, since such recall the moment after a word has been seen is more effective for memory than when the time for it is all spent on perception. The optimum condition with reference to these last two factors seems to be that relation of exposure time and interval for immediate recall that will permit the observer to just complete the processes that for him enter into each.

[20] Huey, 'Physiology and Psychology of Reading,' *Am. Journ. Psych.,* Vol. XI. Also, *The Pedagogy and Psychology of Reading,* New York, 1908, by the same author. Dearborn, 'The Psychology of Reading,' *Archives of Philos., Psych. and Sci. Meth.,* 1906.

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ON THE ANALYSIS OF THE FACTOR OF RECALL IN THE LEARNING PROCESS.

BY EDWINA E. ABBOTT.

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I. The Nature and Aim of the Problem.

No one who has had experience in memorizing or in watching others memorize can have failed to observe the tendency of the average person when he is reading, to momentarily turn away from the material before him and to repeat it to himself without external aid. This tendency to recall appears whatever may be the material which is to be memorized or to whatever sense it is presented. The learner will often go through this procedure even when he knows he cannot remember the whole accurately; or, if the material is easily grasped, after he is able to repeat it perfectly and before dismissing it from his attention, he will repeat it to himself two or three times. The fact that this tendency is to be observed, however, does not justify us in believing without further evidence that recall is a desirable and helpful factor in the learning process. For, if experimental results in the field of memory have demonstrated anything, it is that the average person is not sure to use in memorizing the best and most economical methods to help himself. It is only by careful investigation under all conceivable conditions that we may obtain any adequate idea of the use and relative value of these factors which appear in the mental habits of normal people.

Certain results obtained in an experiment 'On the Analysis of the Memory Function in Orthography '[1] conducted in the psychological laboratory of the University of Illinois in the year 1907–08, led to the conclusion that the opportunity for recall, during or immediately after the learning process, was of great benefit to the individual. It has been the aim of the present experiment to determine more carefully the nature of the influence of this recall and the conditions under which it could be used most favorably. In other words, to determine, when a definite length of time is given in which to learn a given amount of material, whether it is of the greater advantage to spend all the time in actual perception of the material, or part of the time in perception and part in recall; and also whether the recall should be interspersed with the perception or should follow it immediately or after an interval. We have also attempted to make some analysis and explanation of the factors which are of influence in this recall period and particularly to determine the effect of localization.

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[1] See paper in this monograph.

The work already done along the line of economy of learning has shown that the relative value of many of the so-called methods of learning depend in a large measure on the memory type of the individual who is to use the method. A condition which would be of great advantage to a visually-minded individual might prove positively distracting to one of the auditory-motor type and again might have no appreciable effect on the individual of a mixed type. In view of this, in the present experiment care was taken to determine from introspective analysis the type of imagery of the subjects and in general the mental processes which they went through in learning the material presented to them.

II. LITERATURE RELATED TO THE PROBLEM.

In the experiment under discussion the total time devoted to learning was divided into two equal periods and separated by varying intervals. This procedure relates itself to the experiment of Jost,[2] who endeavored to verify the conclusions of Ebbinghaus.[3] Ebbinghaus found that sixty-eight successive repetitions of memory material have a less profitable effect than thirty-eight repetitions divided over three days. In his experiment Jost used the 'Esparnismethode' to determine whether there was greater or less advantage in presenting a series of twelve nonsense syllables thirty successive times, or in dividing the thirty repetitions equally over three days. His results showed that twenty-four hours after the final repetition there was more time saved in relearning when the first thirty repetitions had been divided. In an attempt to discover the reason for this superiority Jost investigated the laws of association by means of both the 'Treffer' and 'Esparnismethode' and comes to the following conclusions: First, that only the 'Treffermethode' can give a direct determination of the reproduction tendency of a series; second, that the two methods cannot give results at all comparable; third, that there are two fundamental laws of association as follows:

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[2] 'Die Assoziationfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen,' *Zeitschr. für Psychol. u. Phys. d. S.*, XIV., 436–472 (1897).

[3] *Ueber das Gedächtnis*, Leipzig, 1885.

1. If two associations are of the same strength but of different age, a new repetition will have the greater value for the older one.

2. If two associations are of equal strength but of different age, the older one will decrease the less with time.

In all memory experiments in which the same material is presented repeatedly the factor of localization must enter to a greater or less degree. Pohlmann in experiments with school children and seminary students, in which nonsense material was presented with and without the opportunity for localization, concluded that this factor is an aid to memory and also that it is not necessarily of a visual character although apt to have more influence when of a visual nature.

The experiments of Witasek[4] and Katzaroff[5] bear more directly on our problem. Their purpose was to determine the relative effect of recitation and reading. Witasek presented groups of ten nonsense syllables by means of Wirth's memory apparatus at the rate of one syllable per second. The subject read the syllables aloud in trochaic rhythm during those series designated as 'reading-series.' During the 'recitation-series' the syllables were pronounced aloud and the subject attempted to keep the rhythm and tempo the same as in the reading series. When the syllable was forgotten the subject was given ten seconds in which to recall it. If he failed to recall it during this time, *E* read the correct syllable to him and the recitation was resumed. When a wrong syllable was given the subject was stopped and the right one given him. The subjects were instructed never to go back over the syllables either during the reading or recitation series and to rule out associative links as far as possible. The order of series was as follows:

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[4] 'Über Lesen und Rezitieren in ihren Beziehungen zum Gedächtnis,' *Zeitschr. für Psychol. u. Phys. d. S.,* XLIV., 161–246 (1907).

[5] 'Le rôle de la récitation comme facteur de la mémorisation,' *Archives de Psy.,* VII. (1908).

	A.	B.	C.	D.
(*a*)	XVI 5	VI 0	XI 10	XV 10
(*b*)	XI 5	VI 15	VI 5	XI 0
(*c*)	XVI 0	XI 15	XX 0	VI 10

The Roman numerals indicate the reading series, the Arabic numerals the recitation series. A, B, C and D were alternated to eliminate practice effect. In every case ten 'test-recitations' were given one hour after the completion of a series. A record was kept of the time consumed by every recitation and also of the helps given in the course of every recitation. From these data the following conclusions are drawn:

1. Recitations are in general far superior to readings in learning value.

2. Of recitation groups following one another the first has the greatest learning value and each succeeding group has less.

3. Of readings following one another the first has the greatest learning value and each succeeding reading has less.

4. The learning value of a recitation is higher if the association by which it is strengthened is made not through single readings but through reading and recitation.

Katzaroff used in his experiment the 'Treffermethode.' He presented groups of eight or ten pairs of nonsense syllables on a revolving drum which showed each couple for two seconds. Successive readings were separated by an interval of four seconds. During the presentation the subject was required to read the series aloud in trochaic rhythm. He was not to create artificial associations. During the recitation the first syllables of every couple were presented in a different order from that of the reading presentation. The subject gave the corresponding syllable as quickly as possible. If he was unable to recall it or gave it wrongly, the right one was given to him. The series were presented with the following combinations of readings and recitations. The column to the left indicates the number of 'fundamental readings'; L signifies reading and R signifies recitation. The small number indicates the number of successive readings or recitations in a group.

			Final recall after
L 10	interval of 2 minutes	R1 L5	48 hours.
L 10	interval of 2 minutes	R1 R5	48 hours.
L 8	interval of 2 minutes	L7	72 hours.
L 8	interval of 2 minutes	R7	72 hours.
L 8	interval of 2 minutes	L7	72 hours.
L 8	interval of 2 minutes	R1 L6	72 hours.
L 4	interval of 2 minutes	L6	24 hours.
L 4	interval of 2 minutes	R6	24 hours.
L 4	interval of 2 minutes	R1 L1 R1 L1 R1 L1	24 hours.
L 4	interval of 2 minutes	L6	24 hours.
L 4	interval of 2 minutes	R3 L3	24 hours.
L 4	interval of 2 minutes	R2 L1 R1 L1 R1 L1	24 hours.

Katzaroff concludes that:

1. All the experiments show R superior to L in conservation, promptness and certainty of memory; we are justified in concluding that after a certain number of fundamental readings the recitation has a fixing value superior to that of an ordinary reading.

2. The extent of R's superiority varies according to the number and disposition of R in relation to L. The best combination appears to be a number of fundamental L's proportioned to the length of the series. Then at least two R's, then at least two L's to reinforce the series as a whole.

Katzaroff explains his results: first, by the fact that the mental attitude in recitation is active and one of constantly looking forward and never looking back, while in reading the attitude is passive, thus making recitation more favorable for the factors of attention and the richness of association; and second, from the point of view of the cerebral force. The neural paths which are traced in the trial recitations are the same as those used in the final recitation. The centers and paths used in reading are not the same.

Meumann[6] has noted this natural tendency, referred to above, to make use of an immediate recall in connection with the learning process. In treating that phase of memory which constitutes the attentive impressing on the mind of sensory data, he states that "all sense impressions go through a double period of forgetting. The first is, in a measure, only a fading of the original impression and during this period the impression will be reproduced most easily and similarly will be most easily made enduring by a repetition of the stimulus. Therefore we see that it is important for the technique of the learning of sensory material that the active impression be made while the stimulus is still present or immediately thereafter. We proceed thus unconsciously when we wish to make a lasting impression on our minds of some complicated material. We reproduce it while the object is still before us or we close our eyes a moment and then bring our attention back to the object after we have endeavored to reproduce it unaided."

It is obvious that Witasek and Katzaroff in introducing recitation into their period of recall made of it an entirely different process from that described by Meumann. The factor emphasized by Katzaroff in his explanation of the efficacy of recitation is the attitude of 'never looking back and constantly looking forward.' This factor is in direct opposition to that of 'immediate imaging' by which Meumann explains the value of immediate recall. Since it is obvious that both these factors cannot enter at once into the mental process of a subject who is given an opportunity for immediate recall, the question remains, which factor is used when the subject is left free to use the recall period as he likes and which is of the greater value for learning. It shall be our purpose to show that the use and relative efficiency of these factors depend on the memory type of the individual who is doing the learning.

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[6] *Ökonomie und Technik des Gedächtnisses,* Leipzig, 1903.

III. Method of Procedure.

The method used in conducting the present experiment differs in a few particulars from that used in preceding experiments bearing on the same problem. Other experimenters have required their subjects to make their recall entirely in the form of recitation. This would necessarily bring the vocal and auditory factors into strong prominence with all subjects and the results would in all probability be influenced by the individual type of the subjects. Since it was the purpose of the experiment under discussion to discover the factor or group of factors to which the influence of the recall period might be traced, the subjects were given the time for recall without directions as to how it should be used and through introspection the attempt was made to discover the factors present.

Katzaroff in his experiment on the influence of recitation used the 'Treffermethode,' that is, he presented nonsense syllables in pairs for the learning series, and for the recitation and recall series he presented only the first of each pair and the subject was instructed to give the accompanying syllable. It would appear that in seeking for the accompanying syllable the subject has two distinct courses open to him. He may remember it by means of its association with the syllable he is looking at; or he may remember it by some association in the column of syllables to be recalled. Since these two courses may introduce a complexity of factors into the recall we preferred to use in our experiment the more simple method of recalled members.

The most noticeable difference in our method from that of other investigators lies in the fact that in no other experiment has there been any definite attempt to determine the individual type of the subjects and to use this knowledge in the interpretation of the numerical results. So much has been said and will appear later concerning the importance of this knowledge that it is sufficient to state here that throughout our experiment this determination has been done in the case of every subject with the greatest care.

The material used in the experiment was of two kinds, nonsense syllables and words. The nonsense syllables were made up according to the method of Müller and Schumann and presented in groups of thirty. The words were chosen at random from an English dictionary and included nouns, verbs and adjectives. They were never over four syllables in length. They were presented in groups of sixty. Both words and syllables were typewritten in columns on sheets of thin white cardboard and were so spaced that when the cardboard was fitted into the slide of a 'Jastrow fall shutter,' the words or syllables were exposed before a narrow opening, either one immediately after the other, or with a blank interval between. This fall apparatus was fitted with an electro-magnetic release. The electric circuit which controlled the falling shutter contained a mechanical interrupter or contact mechanism in the form of a toothed wheel. By the introduction of wheels of different forms, containing different numbers of teeth and different distances between the teeth, it was possible to vary the temporal relation of the exposure at will. Thus the stimulus word or syllable could be exposed for one second with an interval of three seconds or with no interval. This mechanical interrupter was in turn controlled by an electric pendulum (Zimmermann's *Kontaktuhr*). The word or syllable when it was exposed on the shutter was reflected through a lens onto a ground glass screen in a large camera-shaped box. At the end of the box opposite the lens was an opening of a convenient size and the subject sat before the box with his eyes on a level with this opening. The box was so made that the focus could be adjusted for any individual. The system of lenses had a diameter of five centimeters and a focal length of thirty-two centimeters.

It will be seen from the table on page 167 that in series a, b, c, d, e, f, h, i and j, sixteen minutes were devoted to the entire process of learning, while g and k received only eight minutes. Also that in the series a, b, c and g there is no opportunity given for recall, the learning periods being consumed in *Einprägung;* that in series d, e and f half the time is given to recall and it comes after the *Einprägung* and that in series h, i, j and k three fourths of the time is given to recall and it is interspersed with the *Einprägung* during the three seconds interval between presentations.

These series were presented in two different ways. The first way, which we have termed the ‘regular order,’ consisted in keeping the order of words or syllables in a group constant throughout the presentation time. The second way, termed the ‘mixed order,’ differed from the former in that a group of words or syllables was never presented twice in the same order.

The groups of words and syllables were presented in eleven different kinds of series as follows:

Series.	First 8 minutes spent in	Exposure time per word or syllable.	Interval between words or syllables.	Interval between 1st and 2d learning period.	Second 8 minutes spent in
a	visual Einprägung[7]	1″	0	1′	Einprägung
b	"	1″	0	15′	"
c	"	1″	0	45′	"
d	"	1″	0	1′	Recall
e	"	1″	0	15′	"
f	"	1″	0	45′	"
g	"	1″	0		
h	Einprägung and recall	1″	3″	1′	Einprägung and recall
i	"	1″	3″	15′	"
j	"	1″	3″	45′	"
k	"	1″	3″		

In both series the final recall was made four hours after the learning was completed. The subject was required at this time to write down all the words and syllables he could remember. He was given fifteen minutes for each group and at the end of that time those correctly recalled were counted and recorded. Only one group of words was given during one day to each subject. The series were not given in the order described, but in such an order that practice effect would be equally distributed for the three different sorts of series; that is, for the a, b, c group, in which there was no opportunity for recall, the d, e, f group in which the recall all came after the *Einprägung* and the h, i, j group in which the recall was interspersed with the *Einprägung*. During the one, fifteen and forty-five-minute intervals the subjects were not allowed to think of the material. The fifteen and forty-five-minute intervals were usually consumed in taking other groups or in presenting groups to another subject.

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[7] ‘Einprägung’ is the German word which designates the process of attending to sensory materials with a view to remembering it. There is no term in English which so aptly describes the process, and we have used the term ‘visual Einprägung’ to designate the time spent by our subject in attending to material visually presented with the view to remembering it.

The subjects were given certain instructions. During the presentation of the groups in series a, b, c, d, e, f and g the subject was not to form any associative links between the words or syllables and while one word or syllable was before him he was not to think of any other. We endeavored in this way to prevent as far as possible any attempt at reimaging. During the recall period in the d, e, f series the subject was allowed to recall the group in any terms of imagery he wished and to associate the words or syllables in any way he wished. But he was not allowed to pronounce them aloud or to write them down. The recall period of three seconds in series h, i, j and k the subject was allowed to spend as he wished, and also in these series he was allowed to form any associations he wished to between the different members of the group. He was also instructed not to think of the material during the period between the learning and the final recall.

Throughout the experiment careful introspections were taken of the processes and imagery used both in the learning and recall. These were supplemented by questions from the experimenter with the particular object in mind of throwing light on the individual type of the subject. The subjects were students from the psychological laboratory at the University of Illinois and were five in number.

IV. RESULTS.

1. *Introspective Results.*

The numerical data when interpreted in the light of the introspective results take on an entirely new significance from that which appears when they are studied by themselves. Hence it is of the greatest importance before presenting the actual results of the experiment, to give a complete description of the type of procedure of each individual as it has been disclosed by means of the subject's own analysis. The procedure in the case of *Einprägung* seems to differ very slightly for all the subjects. They all look at the word or syllable attentively. Most of them are conscious of whispering the word or of employing 'inner-speech.' One of a marked visual type, however, repeatedly asserts that she is unconscious of any pronunciation whatever. She simply 'looks at the word and lets it soak in.' When one word passes from the field, the attention is focused on the next that appears.

When we come to the recall in the d, e, f series, in which half the total learning time is given for recall and comes after the presentation is completed, we find that each individual constructs what he can of the group in terms of imagery most natural to him and then spends the remaining time reimaging and in forming more or less artificial associative links between the words or syllables. It is significant that these links are said to be formed after the first reimaging. The only exception in this general course of procedure is found in the case of *Y* (strong visual type), who merely reimages repeatedly and does not form associative links between the words or syllables.

It is when we come to the h, i, j group, in which the recall is interspersed with the *Einprägung* that we find the greatest deviation in general procedure. Four of the subjects spend the interval of three seconds of recall after each term of the group in thinking of the term which has been seen, in reimaging it according to their several types, in imaging some concrete object, perhaps sometimes in running back over the last two or three terms seen. For these cases the procedure is clearly that described by Meumann and referred to earlier, namely, that of catching the image just as it is fading from consciousness after the actual presentation, when it may most easily and most accurately be revived, and impressing it upon the mind. During this period there are also to some extent associative links formed, such as words and phrases which fit together by meaning, or rhyme or jingle. These are repeated together and they thus fall into groups. This associative tendency we find lacking, however, in subject *Y*. This subject reimages the last term usually and with few exceptions in concrete visual form, but there is little or no association between terms. Entirely different from this kind of procedure is that of the fifth subject, *W*, whose introspections follow in his own words. "The process of learning in this kind of a series is thus,—The first time the list of words is seen for each word, some image is called up and connected with the image of the next word. The word is also pronounced. After the first presentation, as soon as one word has disappeared I try to think of the next word. If I can, I say it over to myself. If not, I wait for it attentively. I put equal time on every word, for I never run back or forward in groups but always think of the word which is to follow immediately. The process is the same for the syllables, except that there is no concrete imagery for them outside the visual imagery of the syllable itself. These are sometimes spelled through and sometimes pronounced." This method of learning was possible only in the 'regular order.' In the 'mixed order' this subject re-imaged the last word presented just as the other did. It will easily be seen that in the 'regular order' series this subject does not use at all the immediate reviving of the image of the term just seen, which revival is of such importance to the others. He has literally transformed the recall series into a recitation series such as those used in the experiments of Katzaroff and Witasek, the only difference being that he 'recites' in the imagery which he is most accustomed to using in his thinking, instead of in vocal and auditory terms, which he may or may not be accustomed to using. These general distinctions given, we will present the more detailed descriptions of the individual type of the five subjects and their procedure in the different kinds of series.

V. This subject makes use both of 'inner-speech' and concrete visual imagery in his thinking. In the a, b, c and g series, when there is no chance to think the words together, he pronounces them and looks at them attentively while they are being presented. Sometimes the concrete visual appears. In the final recall there is an attempt to revive whatever imagery there may be left of the word or syllable, but the subject seems to have poor control over this imagery. When trying to recall, he feels that the words are there and that they would be recognized instantly if presented. They seem to be just on the threshold, but there is nothing to get hold of them with. There is no handle. The predominant character of this imagery seems to be visual. "There seems to be a visual image," he says, "but it is one that moves very fast and there is not time to get any one word." Again, "In all cases of the mixed order the visual image of the word is used exclusively. The word is always pronounced when seen but this seems to be no aid in final recall." During the recall period in the d, e, f series, the subject states that the visual image and inner pronunciation of those terms of the group which are recalled, appear simultaneously and all the terms which can be recalled are then linked together by some meaningful association. The process in the h, i, j and k series has already been described. It would appear then that the general character of *V's* imagery is visual and also that the mere mechanical reproduction of sensory data in terms of imagery is difficult for him.

W. This subject is of a distinct verbal type. We do not find the concrete entering to a marked degree and when it does it appears to accompany and not aid either the learning or recall. This verbal imagery is motor-auditory and the *Einprägung* of a word or syllable is always accompanied by pronunciation. The imagery used in the recall period is decidedly of a mixed type and is in the main verbal. The process of learning seems to be one of reinforcing the verbal impression with various kinds of imagery, occasionally associating it with some concrete image or with some other word or syllable in the series and usually localizing it in the series either visually or by motor aid.

X. This is a subject who is predominantly motor-auditory, but who also uses visual imagery to a large extent. In the h, i, j series the recall interval is used for repeating the term last seen and 'so fixing it in my mind.' Visual localization is employed frequently and if the first term of a column is gone, the other terms in that column are often lost entirely.

Y. This subject is of a very strong visual type who 'never pronounces a word or syllable in learning, merely tries to absorb the visual picture.' Concrete visual imagery also appears often. The words and syllables suggest these concrete images, but are rarely linked with other words or syllables in the group. During the recall period the last term seen is visualized and sometimes the last several terms are run over. In the final recall the subject remembers them ' by seeing them in the mind and remembering them as they appear on the screen.' The ' mixed order ' impressed this subject as being particularly unfavorable. The concrete visual appears much more in evidence in the ' mixed order,' as the serial localization is inhibited.

Z. This subject was given only a few series, to determine if possible the effect of localization, and is a remarkably clear case of the necessity for introspective evidence as a means of interpreting objective results. The procedure was as follows:

For the mixed order the words are classified according to meaning and syllables according to resemblance and " the whole feeling is one of trying to get them through as many of the senses as possible." Concrete imagery rarely enters, although it sometimes does and is always a help when it does.

In the regular order all these aids are thrown overboard completely and a visual map of the card is made. In the recall visual imagery is used exclusively. These results show how completely the change of one objective factor may change the entire mental attitude with which the subject attacks the problem and cause entirely new processes to be used. It is folly to suppose that our mental processes are made up of factors which bear a definite relation point for point to the objective factors of the experiment and that by changing one objective factor we will merely change a corresponding factor or group of factors in the mental process. On the contrary we may by that one objective change substitute an entirely new set of factors in the mind of the subject.

2. *Objective Results.*

The tables below are given in terms of the per cent. of correctness.

Table I. presents the various series as described above for both the regular and the mixed order. The subjects tested are indicated at the right, each subject being tested on both words and syllables, and subjects *W*, *Y* and *Z* are tested for both the regular and the mixed order.

TABLE I.

Series.	Regular Order.											Subject.
	a	b	c	d	e	f	g	h	i	j	k	
Words	23	23	24	23	33	25	23	41	59	42	8	*V*
Syllables	42	38	40	35	40	17	22	68	92	70	47	
Words	8	8	13	29	12	8	5	97	98	97	62	*W*
Syllables	20	15	12	20	30	27	8	97	97	98	62	
Words	15	28	23	28	15	18	13	31	34	30	18	*X*
Syllables	13	13	22	22	17	18	5	30	27	48	10	
Words	67	78	83	48	42	43	28	53	55	17	23	*Y*
Syllables	67	53	70	67	63	50	57	63	67	53	43	
Words	54			61				56				*Z*
Syllables	60			50				55				
	Mixed Order.											
Words	10	13	17	13	12	7		20	17	8		*W*
Syllables	17	23	27	40	20	17		10	13	7		
Words	28	25	20	33	17	17		27	18	27		*Y*
Syllables	67	10	40	40	37	20		30	43	37		
Words	55			49				53				*Z*
Syllables	52			68				62				

TABLE II.

Series.	a	b	c	d	e	f	Subject.
Words	0	0	1	0	10	2	*V*
Syllables	20	16	18	13	18	—5	
Words	3	3	8	24	7	3	*W*
Syllables	12	7	4	12	22	19	
Words	2	15	10	15	2	5	*X*
Syllables	8	8	17	17	12	13	
Words	39	50	55	20	14	15	*Y*
Syllalbes	10	—4	13	10	6	—7	

Table II. was derived from Table I. by substracting the results obtained in series g, from the results obtained in series a, b, c, d, e and f, respectively. This second table, therefore, represents what is gained by the added eight minutes of learning or recall over the time taken for the simple *Einprägung*. In some instances there is a minus quantity. Only the regular order was so treated.

Table III. gives the results of the words and syllables combined, as shown separately in Table I., but for the regular order only.

Table IV. gives a comparison of the regular and of the mixed order for syllables and words combined and was derived from Table I. The letter T in Tables III. and IV. indicates the average of the totals for a, b and c; d, e and f; and h, i and j, respectively.

TABLE III.

Series.	a	b	c	T	d	e	f	T	h	i	j	T	g	k	Subject.
	29	28	29	29	27	35	22	29	50	70	51	56	23	21	*V*
	12	11	12	11	26	18	14	19	97	98	97	97	6	62	*W*
	14	23	23	20	26	16	18	20	31	32	36	35	10	16	*X*
	67	70	79	72	54	49	46	49	57	59	29	48	38	30	*Y*

TABLE IV.

	a	b	c	T	d	e	f	T	h	i	j	T	Subject.
Regular order	12	11	12	11	26	18	14	19	97	98	97	97	*W*
Mixed order	12	17	20	16	22	14	10	15	17	16	8	13	
Regular order	67	70	79	72	54	49	46	49	57	57	29	48	*Y*
Mixed order	41	50	27	39	36	23	18	26	28	27	30	28	
Regular order	56	—	—		57	—	—		56	—	—	—	*Z*
Mixed order	54	—			56	—	—		56	—	—	—	

In comparing the efficiency of memory for words and syllables, we would expect for several reasons to find the syllables possessing an advantage over the words. Inasmuch as the groups of words were twice as great in number as the groups of syllables and the time of presentation was constant, each group of syllables was actually presented twice as many times as its corresponding group of words. The syllables, too, are shorter and simpler to perceive. In general we find the objective results (Table I.) show that this advantage exists for the syllables. The exception we find where we would expect to, namely, in the results of *W* for the h, i and j series, where imagery played an insignificant rôle in the learning process. We would also expect that this superiority would be most marked in the case of *Y*, our strong visual subject, who uses imagery exclusively in learning and here again to an extent and particularly in the mixed order the objective results justify our expectations. In this case, also, it is significant that the superiority decreases as the number of presentations of both words and syllables increases.

Comparing the one, fifteen and forty-five minute intervals between learning periods (Table I.), we obtain no conclusive results except in the d, e and f series where there is some uniformity in favor of the one minute interval. One subject, however, finds the fifteen minute interval more favorable. We may say in the main that when the recall comes after the *Einprägung,* the sooner it comes the better.

When we compare the results for g with those for d, e and f we obtain exactly the amount the recall adds to the permanent effect of eight minutes of *Einprägung* (Table II.). Although there is no uniformity the striking point is that there are only three cases out of the twenty-four in which *something* is not added and that for *V, W* and *X* in the majority of cases the recall adds as much as or *more* than the added eight minutes of *Einprägung* in *a, b* and *c.* As we would expect we find that in the case of *Y*, although the recall adds something, it is less than what the same amount of additional *Einprägung* adds.

The most significant comparison lies in the figures for the three groups a, b, c; d, e, f and h, i, j (Table III.). In subject *W*, who as has been explained before, makes of this kind of series a mental recitation we find an immense advantage for h, i, j over a, b, c or d, e, f. For *V* and *X*, who use associative links, or rather groupings, to some extent to reinforce their imagery, we find the h, i, j series has a distinct advantage over the a, b, c and d, e, f series, but this advantage is slight. For subject *Y* who uses little or no associative help but depends on strong visual imagery, we find the h, i, j series is distinctly unfavorable and those series in which the groups are seen the greatest number of times have a great advantage.

Comparing the effect of the mixed order with that of the regular order (Table IV.), we find in *Y's* results the mixing of the order has an unfavorable effect and this effect increases as the number of actual presentations increases. It is natural in the case of a pronounced visual type such as *Y*, that the more the order of presentation is changed the more confused he would become. On the other hand in the a, b, c series of *W* the mixed order seems to have a slight positive advantage over the regular order. This may be due either to a more strained attention or to practice effect. In the d, e, f series we find a slight advantage in favor of the regular order. In the h, i, j series, however, the advantage is overwhelmingly in favor of the regular order. It should be noted again that the procedure in the h, i, j series, which is of such a great advantage to *W* in the regular order, is impossible in the mixed order.

In studying the results of the tests with *Z* who, it will be remembered, changes the whole process of learning to suit either the mixed or regular order of presentation, we find that she apparently adapts herself to the mixed order with as little effort as to the regular order.

Above all, the tables show the consistent and characteristic manner in which the memory determines and explains the efficacy of a given factor in the learning process. That the opportunity for recall is of aid in learning we cannot doubt, but the extent of this aid and the explanation of why it is an aid differs widely for different individuals. For some individuals the opportunity to recall is used mainly to reimage, and as we find this is an aid to memory we must agree with Meumann in emphasizing this immediate retrospect as a basis for the explanation of the efficacy of recall. Another individual, however, may discard reimaging entirely and use the recall as a means for an 'inner-speech' recitation, and as we find this of great assistance in learning we must agree with Katzaroff in his emphasis on the value of the 'mental attitude of constantly looking forward.' Again we must note that that extreme type in whose learning processes we find relatively slight traces of association is more benefited by repeated presentation than by the opportunity to reimage, while for the opposite type repeated presentations seem to have little effect. We must go back to the type of the individual to explain the processes and relative efficiency of this factor of recall.

V. CONCLUSION.

We are led then both by our introspective and objective results to conclude:

1. That the factor of recall is always an aid in the learning process.

2. That when recall comes after the *Einprägung* of the material, immediate recall is of more value than delayed recall and its value decreases as the delay increases in length.

3. That the recall is of greater value when it is interspersed with the *Einprägung*.

4. That localization is one of the factors which go to make recall an aid to memory, but that the relative importance of this factor is determined by individual type.

5. That the relative value of recall and *Einprägung* depends on individual type; that for the individual of strong 'inner-speech' tendency who depends little on immediate re-imaging and in recall goes through the same processes that one would in recitation, the recall is of great value and the *Einprägung* of comparatively little importance, while for the type which depends on immediate reimaging during recall the *Einprägung* is of relatively greater importance than the recall in proportion as the factor of association is less effective in the learning process.

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EDITOR'S PREFACE

This paper represents a piece of coöperation between the Psychological Laboratory of Ohio State University and the Columbus State Hospital. While the author and the editor are responsible for the plan of the experiments, the execution of the work, and the conclusions reached, the superintendent and staff of the hospital have a share in whatever of merit there is in the work, on account of the cordial spirit of helpfulness which they showed while the work was in progress. Without their aid such a study from this laboratory would not have been possible.

AN EXPERIMENTAL STUDY OF DEMENTIA PRÆCOX

L. C. GATEWOOD, M.A.

INTRODUCTION

In beginning a careful study of any disease the question of first importance is, "What is this pathological condition under consideration? What is its scope and what its limitations?" Though there are many definitions of dementia præcox which may serve to partially answer this question, there is none which is clear, concise and at the same time complete. Investigation has shown that there are no hard and fast lines which may be laid down as definitely marking the limitations of this remarkable disease. Though there are recognized various disturbances of the fundamental psychic processes with their characteristic symptoms, these are not such as lend themselves readily to the formation of a satisfactory definition. It is the purpose of this paper, not to formulate a new definition, but to describe and discuss an experimental study of the disturbances of some of the fundamental psychic processes as found in dementia præcox patients at the Columbus State Hospital for the Insane[1], and to compare these processes in the abnormal with the same processes in normal persons.

The first work of this sort was done by Kraepelin and we are indebted to him for the concept, 'Dementia Præcox.' In the formation of this class he seems to have combined three groups of cases each of which was already described and named.[2]

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[1] I wish to acknowledge my indebtedness to the Medical Staff of the Columbus State Hospital for the Insane and particularly to Dr. Stockton, the Medical Superintendent, for the many privileges allowed me, and to Drs. Gaver, Williams and Brundage for their valuable assistance in the selection of cases and the compiling of case histories.

[2] Defendorf, "Clinical Psychiatry" (Dementia Præcox), p. 219.

Later investigation has confirmed the keen insight which placed them together and has minimized the importance of the sub-grouping. It is impossible to make clear differential diagnoses between sub-groups in many cases and this classification, though in some respects adapted to hospital work, is quite inadequate from a psychological standpoint. While some cases may fit perfectly into one of these types, the majority do not and it is rarely possible to say definitely that Case X is one of pure hebephrenia, that Y is catatonia or that Z is one of the paranoid forms. This is well illustrated in some of the cases herein reported. Thus the original diagnosis in Case L. D. J. was hebephrenia but within a few weeks negativism appeared and soon the patient showed stereotypy and, in fact, nearly all of the symptoms of catatonia. Some months later the stereotypy had disappeared and in its place the patient exhibited marked unstable delusions of a paranoid character. Another patient showed the usual hebephrenic symptoms and with them well-marked paranoid delusions, while a third so-called hebephrenic had catatonic states with cerea flexibilitas, paranoid delusions and also some of the characteristic symptoms of paresis.

As a result of the difficulties of this classification dementia præcox has come to be studied, not as a group containing three distinct diseases, but as an entity, a disease complex resulting from the combination of a number of comparatively simple disorders, varying slightly in different cases, but each of which is well defined and has its own part in the final result, the progressive mental deterioration which Kraepelin has named as the most prominent symptom of dementia præcox. Working on this theory I have made a study of the disturbances of attention, association, perception, memory, judgment, volition, motor efficiency and emotional states and have compared the working of these processes in patients at the Columbus State Hospital with the same processes in persons supposed to be normal.

The patients were studied in the different wards and notes were kept in which unusual or significant facts were recorded together with some typical bits of conversation illustrating various phases of a patient's mental condition. The history of each patient was traced as far as possible, especially as regards history of family tendencies, alcoholism, etc., history of onset of the disease and of any previous attacks. For the purpose of obtaining more exact data concerning the course of the disease and its effect on the various mental processes—data which would themselves show the deterioration process far more effectively than those of observation and conversation—I arranged a set of experiments which were used in this work. In order that conditions be constant these experiments were performed at regular intervals, always in the same place and in the same way. They were all very simple. I could not use experiments requiring cumbersome or expensive apparatus, since there is no psychological laboratory at the hospital and there are not facilities for handling or caring for such apparatus there. Because the patients were in different wards and buildings and I deemed it advisable to carry on the work in the various wards, in order to keep the patient as much at ease as possible and thus get a truer measure of his mental condition, it was necessary that my outfit be portable. These and other existing conditions put narrow limitations on the character of the experimental work. The amount of work also was limited by the capacity of the patient. A standard series of tests was used as a maximum. The time required for this series varied from thirty minutes in some normals to two hours in some dementia præcox patients. The order in which the tests were performed was usually the same, though at times it became expedient to change the order or to omit some of the experiments in order to adapt the work to the condition of a given patient. The order in which they were usually used and the order in which they are here described is as nearly as possible the true psychological order.

DESCRIPTION OF EXPERIMENTS

Test I. *Addition Test*

The addition test used was given as a measure of judgment involving memory and attention. The test was of the speed and accuracy in adding and every precaution was taken to eliminate errors due to factors other than the disturbance of the processes to be measured. The sum to be added consisted always of two numbers, one above the other with a line beneath the lower number, all printed on white unruled paper in the same type and spacings as here presented. The number of sums used varied according to the previous training and the condition of the person observed. The maximum used was twenty such sums while five was the minimum. Two numbers of four digits each were generally used, but in some cases numbers of one or two digits were used in order to adapt the test to the ability of the subject and the amount of his school training. This factor remained the same for each patient, throughout the series in most cases, the only exception of any importance being in Case L. D. J., as will be shown hereafter. The time required to complete the test was measured with a stop-watch and was recorded at the time of the test on the sheet with the subject's addition results and also his name and the date. The experimenter marked the errors later. This constituted the record of the test and the sheets were preserved for further use in the study of the relative numbers and kinds of errors made.

$$\begin{array}{r} 6\ 3\ 5\ 4 \\ \underline{7\ 2\ 5\ 9} \end{array}$$

Test II. *Cognition Test*

'Marking out A's.' This test has been used as a measure of the time and accuracy of a simple judgment of cognition. In this experiment the subject was given a sheet on which were printed five hundred capital letters, all evenly spaced in thirteen lines and so distributed that they do not form words or syllables. Of these five hundred letters one hundred were A's, distributed irregularly among the others. The subject was required to mark out, i. e., run his pencil through each A. He was then given a sheet with one hundred A's each in the same position as on the original sheet but without the other letters, and was again required to mark out the A's. The time used and the number of errors made (if any) were recorded on each sheet. It will be seen that the difference between the time required for the first sheet and that for the second sheet will be the time used in finding one hundred A's among four hundred other letters, and one hundredth of this time will be the time required for recognizing an A from the letters among which it occurs. This time varied greatly with different subjects, and for this reason the result of a single test has little significance. The chief value of the experiment is in measuring the variations in a given subject from time to time, and in comparing these variations of the patients with those of normal subjects.

Test III. *Discrimination Test*

This test served well the same purposes as the A-marking experiment. It is Jastrow's experiment for the measurement of reaction time by card sorting. The subject was given a deck of forty cards, of which half had red figures and half had black, (a deck of playing cards from which the face cards had been removed). He was told to deal the cards face downward as quickly as possible, alternating between two piles. The cards were then shuffled and the subject told to deal them face up as quickly as possible, placing each red-figured card on one pile and each black-figured card on another. The time required for each operation was registered and, as in the previous experiment, the difference between the two times is the time required to judge for each of the forty cards whether its figures are red or black. One fortieth of this time is the time required for a single judgment.

Test IV. *Comparative Judgment of Lengths of Lines*

In this experiment a card 125 mm. × 170 mm., having on it a standard black line 100 mm. long was laid on the table before the subject, either at his right or his left as he might prefer, the line being parallel to the edge of the table. Cards of the same size bearing the lines to be judged were then placed before him, one at a time, in a similar position at his left or right, the ends of the two cards (the test card and the standard) being together. The subject was required to judge for each line whether it was longer, shorter or the same length as the standard line. The lines used were 92, 94, 95, 96, 97, 98, 99, 100, 101, 102, 103, 104, 105, 106 and 108 mm. in length and the entire set was used each time. The standard line was always placed on the same side and the comparison was always made at the same distance and the same angle. Only errors were recorded. A serious objection to this test arises from the fact that different occupations develop different degrees of facility in judging lengths and distances. The subjects exhibit very different degrees of proficiency in this direction, making a comparison of single tests of different subjects worthless. The experiment is of value as a measure of the changes in the condition of a subject from time to time by the comparison of his work on different days.

Test V. *Drawing a Line Equal to a Line Seen*

This test is similar to the last, involving the same factors and also the new factor of motor efficiency. A strip of paper 30 cm. long and 10 cm. wide, having at one end a line 100 mm. long, was placed before the subject and he was told to draw, at the other end of the paper, a line of the same length. He was allowed to compare the line he had drawn and to correct it if necessary to make it the same length as the standard line before him. The same sheet is used for the following experiment.

Test VI. *Drawing a Line by Memory Equal to a Line Previously Seen*

The subject's attention was again called to the standard line and the sheet was then turned over and he was asked to draw a line on the blank page equal to the line he had seen. This test thus involves the factors of the Test IV together with the motor efficiency of the preceding test and the additional factor of memory. These line-drawing tests serve the same purposes as Test IV and show less variation. The value of this also consists in comparing with each other results from the same individual at different times rather than in the comparing single tests of different subjects.

Test VII. *Auditory Memory for Letters*

A number of letters so arranged as to form no words or syllables was read to the subject. He then tried to repeat the letters in the order in which he heard them. As a second part of this test a new list of letters was read twice to the subject and he tried to repeat these in their order. The mistakes made and the method used in fixing the letters in memory were always noted, together with the lists of letters used. The number of letters used varied with different subjects and with the same subject at different times in order to adapt the test to his ability with the changes in his condition. With normals a standard ten-letter test was maintained.

Test VIII. *Visual Memory for Words*

The subject was given a number of white cards each bearing a word of five letters. He studied these cards for a short time until he thought he could remember the words. The cards were then mixed with an equal number of similar cards, bearing words of the same kind, names of similar objects, etc., and he was told to separate the word-cards he had studied from those he had not seen before. To aid in maintaining constant conditions the time required for each operation was taken with a stopwatch as in this way the subject used only what time he thought was necessary. The list of words used, the time required for each operation, all mistakes made, and the method used in fixing the words in memory, were all noted in the record of the experiment.

Test IX. *Visual Memory for Pictures*

This experiment was performed in exactly the same manner as the preceding one. The cards used were of the same size (8 cm. long and 5 cm. wide) and each had in its center a simple pen sketch in black ink. The sketches were about 3cm. by 2 cm. The objects represented were domestic animals, carts, boxes, boats, etc., all of which were familiar to every subject. The same records were kept as in the preceding experiment. Particular attention was given the methods used in memorizing the words and pictures used in these two experiments.

Test X. *Motor Efficiency Measurement*

A simple test of motor efficiency was incorporated in this list of experiments since motor efficiency plays a part in some of the other tests used. The test was of the rapidity in tapping on paper with a pencil. A standard ruled sheet was used and the subject tapped as rapidly as possible, making rows of dots back and forth between the ruled lines for thirty seconds. The number of dots made in this time was recorded. As in some of the other tests the difference, even between normal individuals, was so great that the experiment was of value only in the study of the relative variability in individual records and the comparison of this variability in normal subjects with that shown by dementia præcox patients.

Test XI. *Association Test*

This is the association experiment as used by Jung and others. A test word being spoken, the subject speaks the first word or thought coming into the field of consciousness after he perceives the word spoken. The time elapsing between the speaking of the test word and the first motion of the subject to reply was recorded with a stopwatch. The number of words used in a single day varied from 10 to 100. The same words were used with each subject, and my aim was to use as great a number of words as possible and to choose them from widely different fields. This was necessary in order to make the test as near uniform as could be with subjects whose previous training and occupations had been so different. This experiment gives a measure of the speed and accuracy of association. It affords some insight into the disturbance of the processes of memory and judgment. It was also of some value in revealing hidden associations, thus giving evidence of some facts in the history of the various patients that they could not be induced to give in the course of conversation even by the most careful questioning.

Each of the eleven experiments described above was used to measure the disturbance of one or more processes and, since the conditions maintained were constant, the result should be a true measure of the deterioration of the process under study. A comparison of the results from week to week should show the progress of the deterioration of that process. A comparison of the results of all the experiments for a given week would show the average efficiency of the various processes, the general tone of the work for that week and a study of the changes in tone from week to week should show the changes in the patient's mental efficiency. This experimental evidence has been confirmed by the actions and conversation of the patient, and the statements of the physicians and attendants, all of which have been recorded in order to demonstrate conclusively that it is possible to trace the course of the disease in the curves of the experiments. Such a mass of evidence is uninteresting in itself and cannot be presented in outline. It is sufficient to state, that, while in a few cases the curve of a single experiment may not mark accurately the changes in the general mental condition of a patient, the general tone of the experiments for any day will show the condition of the patient for that day, and its changes from week to week will show the progress of the deterioration or the improvement in the condition of the patient.

CASE HISTORIES OF SUBJECTS

Before proceeding with the experimental results obtained, it may be well to give a brief history of each case, describing the most striking psychoses presented by the different individuals and noting the course and termination of the disease in each case. In order that the reader may better understand the curves by which the results of the experiments are presented (Comparison of Dementia Præcox Patients with Normals), I shall give in connection with the first case reported the tabulated record for that case. This type of record was prepared in each case and it was from these records, embodying the complete results of the experimental work done, that the curves of the various tests have been prepared.

Dementia Præcox Patients

Case H. E. M., aged 27 years, single and a native of Ohio. This patient had attended school until about 16 years old, completing the first year of a high school course. He then went to work and soon learned the printer's trade and operated for himself a small printing establishment in his home town until the first signs of insanity were noticed. As is so often the case in dementia præcox, the first sign to be noted was that he was neglecting his work, leaving his shop closed while he was taking trips to neighboring cities with no apparent object. He indulged in excesses and in addition was frequently found shamelessly masturbating. Some delusions of a sexual nature were also noticed. As a result he was taken to a private sanatorium in Columbus, and since he became steadily worse he was soon brought to the State Hospital for treatment. It was after he had been a patient there for some months that I first saw him. He was a man of medium height, was rather heavy set, had dark hair, and his eyes were dark and set well back under bushy black eyebrows. His gaze was restless, never fixing long on one point, and even in a moderate light he blinked often—much as does a man on coming from a dark room into a lighter one. When talking he would never look one fairly in the face, but would look down or off in a corner or out the window. In fact he had a guilty, self-accusing look which plainly bespoke foul habits. On the whole, his face was weak and sensual, showing self-indulgence and lack of will power. His hands, like his eyes, were never still. He would nervously pull at his coat, twist a button or pick at his finger-nails, when he had nothing else with which to engage his stubby fingers. In dress he was at first fairly neat but seemed fond of flashy colors. Later he became careless, slovenly, and, finally, very filthy. He was polite and talked readily and rationally about himself, his home life and his work. He did not realize that he was regarded as insane and believed that he was at the hospital to be treated for a tumor. He exhibited no delusions nor marked abnormal characteristics that would have been noticed by a casual observer. Examination showed however that he had some delusions, and that he had certain well-marked motor disturbances of which cerea flexibilitas was the most striking. He showed no negativism or stereotypy at this time, but as the disease progressed the cerea flexibilitas disappeared and in its place came negativism and stereotypy, illusions and hallucinations. His delusions grew to such an extent that they became the most striking feature of his condition and seemed to form the nucleus around which the other psychoses grouped themselves. Thus at various times he exhibited all the symptoms described by Kraepelin as being characteristic of catatonia (except the stupor). His delusions were often persecutory and were never stable though the same general ideas occurred throughout. They were frequently of a sensual nature and showed his abnormal sexual emotion, which was further evidenced by continued masturbation.

The deterioration was very rapid and at the end of about three months his condition was such that further experimentation was impossible. He then showed marked negativism, stereotypy, loss of memory and apprehension and seemed always completely absorbed in his hallucinations, which were chiefly auditory. Thus he would sit in one position for hours, the muscles of his face twitching and his lips moving at times as he conversed with the voices which he said he heard by 'insulation.' He believed that he had a sweetheart who was staying in a building a few hundred yards away in order to be near him, and he claimed to be able to talk to her and to see her through walls and across space by this power of 'insulation' which she likewise possessed. Likewise there were some other persons who could talk by 'insulation' and who used it to persecute him by telling false stories about him and by calling him names and interrupting him when he wanted to talk to his sweetheart. Thus his delusions centered about these hallucinations and at one time I would find him laughing over something funny he had heard, while at another he would be weeping bitterly because this girl had told him that I had shot his father.

Along with these delusions developed a number of tics—peculiar twitchings of the facial muscles—that he believed were a necessary part of the 'insulation.' These contractions however were of a sort which do not normally occur voluntarily, being for the most part unusual combinations of contractions of the corrugator supercilii, occipito frontalis and orbicularis palpebrarum (producing twitchings of the forehead and eyelid) and of the risorius, depressor anguli oris and levator labii inferioris. In these tics the muscles of both sides did not always work together or equally. Thus one picture (a snapshot) shows a twitch of the corrugator supercilii very much more marked on the left than the right side and at the same time a stretching and depression of the angle of the mouth by the risorius and depressor anguli oris. A picture taken one second later would have shown a different set of contractures since these were not the slow normal contractions of the muscles brought into play which occur as the result of a continued stimulus (volitional) and are known physiologically as tetanus, but corresponded more closely to the true physiological muscle twitch which is produced by an instantaneous stimulation of the nerve supplying the muscle and cannot be produced voluntarily. They did not always accompany either the movements of his lips or the hallucinations of hearing but might accompany either or both. The interference of the hallucinations with the various mental processes is shown in many ways. Thus when listening to the voices he would often fail to hear his name called, or he might know that he had been spoken to but would not know what had been said or asked of him. The retarding effect is shown by the fact that if asked a question he would often sit still for many minutes conversing with the 'voices' but would finally answer the question asked without its having been repeated. Another peculiar disturbance intimately connected with the hallucinations was shown in the repetition of an answer to a question, the interval between the repetitions being completely taken up with his conversation with the 'voices.' For example, when I greeted him one morning he did not answer for a time but finally shook hands with me and replied "Good morning. How are you this morning? I am fine and dandy."

After an interval of about twenty seconds, and without any further words on my part he repeated the greeting and again shook hands. Thus he repeated the same words and actions eighteen times, the interval each time being longer than before and being always occupied with the 'voices.' After the last repetition he became completely absorbed in his hallucinations and gave no sign of hearing or understanding any of the questions I asked him. This condition persisted and made conversation at all times difficult, and sometimes impossible even for his brothers and sisters, who frequently visited him. When he had been in this condition for about two months, that is, at the end of the fifth month of my observations or about one year after the onset of the disease, he was removed to a private sanatorium in a distant part of the state. At the last report of his case a year later his condition remained unchanged except that his dementia was more profound. Thus the disease, in this case, after a gradual onset, ran a course of rapid deterioration with termination in complete dementia.

A study of the family history of this patient reveals no history of insanity of any sort and none of the relatives so far as can be learned are now insane, though many of them show a neurotic condition which may be regarded as predisposing to insanity. Some of the brothers and sisters of the patient were formerly used as subjects in some experiments carried on by an observer from this laboratory and he confirms my statements of a neurotic disposition and further reports that he found them quite suggestible and peculiarly susceptible to hypnotism. Others of the family showed emotional instability, often becoming suddenly and violently angry on slight cause and in other ways giving evidence of a weak, nervous temperament. There is no record of bad habits in either of the parents but the patient himself has led an intemperate life, using alcohol to excess and indulging in promiscuous intercourse. His sexual nature has probably always been abnormal and there is history of long continued masturbation. He denies having had syphillis and there is no evidence of his having had any venereal disease.

The experiments used with this patient were at first the standard tests as used with normal subjects but as deteriora-

Table I (Case H. E. M.)

Tests	Weeks	1	2	3	4	5	6	7	8	9	10	11	12	13
Test I Addition Test	Number of sums	20	20	20	20	20	20	20	20	20	20	20	20	20
	Number of errors	4	3	4	1	3	2	1	2	3	3	3	2	7
	Time of all sums	331	251	240	217	382	273	307	480	335	557	408	530	950
	Time of ten sums	165	125	120	108	191	136	153	240	167	278	204	265	475
Test II Marking Out "A's"	Time marking A's No. 1	132	119	132	155	170	134	141	140	149	129	150	186	360
	Number of errors								1				1	
	Time marking A's No. 2	72	56	63	69	81	64	68	75	64	70	43	89	139
	Number of errors													
	Time finding 100 A's	60	63	69	86	89	70	73	66	85	59	107	99	221
Test III Discrimination Test	Time of simple piling	37	35	37	37	34	45	39	39	34	46	38	45	×
	Number of errors													
	Time sorting for color	49	52	49	57	60	73	56	95	59	72	56	95	×
	Number of errors												2	×
	Discrimination time (40)	12	17	12	20	26	28	17	56	25	26	18	50	×
	Discrimination time (100)	30	42.5	30	50	65	70	42.5	140	62.5	65	45	125	×
Test IV "Lines."	Upper limit (perception)	—	102	—	106	100	103	101	103	—	105	103	—	×
	Lower limit (perception)	—	106	106	106	—	—	—	104	108	—	—	—	×
Test V Lines compared.	Length equal to 100 mm.	102	104	100.5	107.5	108	103	104	107	99	102	108	112	104
	Error	2	4	.5	7.5	8	3	4	7	1	2	8	12	4
Test VI Line memory	Length equal to 100 mm.	97	92	101	109	100	107	104	99	91.5	100	95	100	107
	Error	3	8	1	9		7	4	1	8.5		5		7
Test VII Letter Memory (Auditory)	Number of letters No. 1	10	10	10	10	10	10	10	10	10	10	7	10	×
	Number of errors	8.5	4	6.5	4	5.5	2	4.5	3		1	4	3.5	×
	Number of letters No. 2	10	10	10	10	10	10	10	10	10	10	10	10	×
	Number of errors	3	3.5		1	12.5	4	4	3	4.5	2	3	9	×

Test VIII Word Memory (Visual)	Time studying cards	145	75	20	43	130	125	100	–	75	20	25	15	×
	Time sorting cards	30	25	15	22	65	46	58	–	28	80	40	105	×
	Number of cards used	10	10	10	10	10	10	10	10	10	10	10	10	×
	Number of errors	2	1	2		2	1		1		2	2	5	×
Test IX Picture Memory (Visual)	Time studying cards	65	57	48	50	45	–	47	120	45	48	53	15	×
	Time sorting cards	57	30	35	35	–	–	55	50	45	80	37	46	×
	Number of cards used	10	10	10	10	10	10	10	10	10	10	10	10	×
	Number of errors	1		1	1	5	1	1			2		1	
Test X Tapping	Number of dots made in thirty seconds	179	165	174	180	147	153	145	153	151	163	169	132	152

NOTE.—Time is always given in seconds.

Distances or lengths are given in millimeters.

× signifies that the patient's condition was such that the test so marked was not performed.

– signifies that accurate measurement could not be obtained.

Zeros are not recorded in error columns. When no errors were made the error space was left blank.

In the addition test the time of ten sums was found since this was a convenient standard to which the results with all subjects could readily be reduced to uniformity for tabulation.

In the A-marking Test No. 1 refers to the sheet of five hundred letters and No. 2 to the sheet of A's only.

The calculation of the time of 100 discriminations in Test III was made to secure the ready reading of the time of a single act of discrimination, and for convenient comparison with the results in Test II. The upper limit mentioned in Test IV is the line least exceeding 100 mm. in length which was recognized by the subject as being longer than the standard line. It is the point beyond which he was absolutely sure the lines shown were longer than the standard. The lower limit was the point beyond which he was sure they were shorter. Errors in tests V and VI is difference in millimeters between the standard line and the line drawn.

In Test VII No. 1 the list of letters was read once, and in No. 2 the list was read twice.

tion progressed they were made less difficult as shown in the table of the experiments presented herewith:

Case L. D. J., aged 25 years, single and a native of Ohio. The patient had spent all his life on a farm in an undeveloped district, distant from any cities of even moderate size and had never been on a railway train prior to the time he was brought to this institution. He had received but a poor education, having attended school but three winters. The remainder of his time had been spent in the routine work of the farm. Of the conditions of his home life and his work nothing is known nor have we any facts concerning the onset of the disease. The only data obtainable are those gotten by questioning the patient and their value from a scientific standpoint is, of course, questionable. He states that his habits have always been regular, that he has never been addicted to the use of drugs or alcoholic beverages and that he has never had any venereal disease. He says that the habits of his parents were good and that neither of them used drugs or alcohol. His mother and three of his grandparents died of 'paralytic strokes,' but none of the other relatives have ever shown any signs of 'paralysis' or insanity. His father, his brother and two sisters were all living and in good health at the time he was brought to this hospital. Since that time he has neither seen them nor heard from them.

When admitted the patient was in good physical condition and his mental aberrations were not such as would attract the attention of the casual visitor. He was short and rather heavy and had a round smiling face and a square firm jaw that indicated aggressiveness. He was not talkative and his answers to questions were always concise and to the point. He was polite and obliging but did only what was asked of him as contrasted with Case H. E. M., who was always ready to offer his services (in the early stages of the disease). The ward physician's provisional diagnosis in this case was hebephrenia and for a time he showed no symptoms which would seem to invalidate this diagnosis, but it was not long before the characteristic symptoms of catatonia began to appear. What had at first seemed like mere reticence developed into negativism and the patient refused to answer questions. Ecolalia was found to some extent and this together with mutism and negativism put a stop to experimentation. Afterward he presented the pressure of activity found in the excited states of catatonia, was violent at times and often exhibited stereotyped movements of various sorts. Cerea flexibilitas was never present.

When first admitted to the hospital the patient was perfectly oriented, had fair memory and judgment and showed no marked disturbances of volition or emotion. But with the progress of the disease memory and judgment deteriorated rapidly until after a year in the hospital the patient was completely disoriented, did not recognize anyone he had met since coming to the hospital, and could not remember the name of his home postoffice, the year or day of his birth or any of the fundamental points of his home life or his experiences at the hospital. Correlated with the deterioration of memory and judgment, and accompanying it, came grandiose delusions of an unstable and unsystematized character. The patient believed he had traveled thousands of miles, had been to South Africa, that he was a great hunter, was very rich, and that the hospital was a fine hotel at which he was staying for a few days' rest. He seemed to have lost completely the idea of time and could not tell the day or the month, nor had he any idea of the time interval between ordinary events such as breakfast and dinner. Thus he passed into a stationary condition in which his negativism and stereotypy, his hallucinations and delusions gradually disappeared. In the meantime the patient had lost flesh, and, like most patients of this class, had lost some of his former decisiveness and poise. His movements were awkward and slow, and much of his strength was gone. His old smile was gone and his expression was blank and dazed like that of one who has just been awakened from a deep sleep. Thus he would sit for hours apparently hearing nothing, speaking to no one, and staring fixedly at the floor. This condition has shown little change for a year and will probably persist until some intercurrent disease overcomes the patient. It is the terminal stage of dementia in which most of these cases end.

Case O. O. L., aged 22 years, single and a native of Ohio. Both parents have been dead for many years and the causes of their deaths are unknown. Since their deaths the patient has lived most of the time with an older brother, who, like himself is single and who keeps house for himself on a farm about ten miles from the nearest city. The present attack is not the first for this patient. He has been at this hospital twice before, remaining each time for a few months and then returning to his brother. His condition for the past year has been such that if he had a good home to which he might go he would be discharged immediately. But the physicians do not deem it advisable to allow him to return to the conditions that seem to exist in his brother's home.

The history of the onset in this attack, as in the two preceding attacks, is rather indefinite since no authentic report was made in the commitment. The patient seems to have no idea of the causes of the attacks nor does he remember the events preceding his imprisonment in the county jail ten miles from his home. He remembers in a faint and rather confused fashion what took place at the jail and some of the particulars of the trip to this city, but when questioned on these points does not always tell the same story. This type of onset seems to have characterized each of his attacks. Since he has been at the hospital his memory has improved as have also his judgment and motor efficiency. Though the patient has never shown marked delusions there is evidence that he had hallucinations and illusions in the early stages of the attack. These have entirely disappeared and he has now reached a comparatively normal stage in which he remains without change of any sort. Since reaching this stage he has been employed in one of the kitchens of the institution and has been granted privilege to walk upon the lawn during daylight hours. He is now probably as nearly normal as he has ever been since his first attack some years ago.

Since his condition was already much improved when I began the study of the case the experimental evidence serves only to demonstrate the gradual improvement of the various faculties with occasional short periods of deterioration. Some departures from the usual conditions of the experiments were made necessary from the fact that the patient had attended school but three years, had never learned to add numbers of more than two figures and could not read or pronounce many of the words used in some of the tests. Thus in the addition test numbers of one digit were spoken and he answered with their sum. In the word-memory test it was found that he must spell out each word and that he was not familiar with many of the words used with the other patients. For this reason the words used were carefully picked and were always spelled and pronounced for the patient while he held the card in his hand looking at it and trying to fix it in his memory. Fewer letters were used in the letter-memory test than with other subjects and in one or two other tests minor details were changed to adapt the work to the ability of this patient. But the conditions remained constant throughout the series and the changes made are of little importance in the final consideration and comparison of results since the changes made served only to adapt the experiments to the previous training of the patient and his mental capacity under normal conditions.

Case A. B. H., aged 30 years, single and a native of Ohio. This patient was engaged in farming previous to the time of his commitment and had been at this institution for five years when the experimental study of his case was begun. When brought to the hospital he was very violent and had to be confined to a strongroom for several weeks. Memory and judgment were poor and he was completely disoriented. He now remembers nothing of the onset of the disease, his trip to the hospital or the weeks of his confinement, and, as careful records of such cases were not kept at that time, we can not obtain a complete history of the case. It has been learned however that after some weeks he began to be less violent and better oriented and to slowly recover his memory and judgment. About this time some delusions began to develop. Chief among these was the idea that he must not eat solid food and it was found necessary to tube-feed him. Though the other delusions disappeared with his continued improvement, this one persisted and, learning to tube-feed himself, he continued to refuse solid food. After some months he was so improved that he was transferred to another ward and was assigned certain duties in connection with the care of the ward. These duties he performed faithfully and he was soon rewarded with many privileges that could not be granted to the other patients. Thus he continued for the six years during which he was a patient at the hospital and in the last year of his residence there, the year during which I studied his case, he showed no striking symptoms other than the delusion regarding his food and this was slowly disappearing. During all this time he seemed anxious to go home and asked the physicians for his discharge a number of times. Each time he was told that as soon as he would begin to eat food as other people did he could go home. But even such an inducement as the chance to go home would not bring him to eat solid food and he continued to use his tube. At last he decided to run away and go home and he did so one afternoon, taking his tube with him. That he should do this when by eating solid food he could obtain permission to go home and run no risk of being brought back is characteristic of the condition of such patients. Here we have a man who has sufficient judgment to plan an escape and to make his way home, a distance of about seventy miles, quickly and safely and without attracting sufficient attention to cause his apprehension, a man who, to the casual observer, would appear quite normal, as he must have to those whom he met on his trip home. His delusions have so far disappeared that there is no longer evidence of them and he can assign no valid reason for not eating solid food. The use of the tube is simply a habit now. It was formed when the patient had a reason satisfactory to himself for not eating solid food and the habit persisted after that reason had disappeared because the patient's judgment was not strong enough to see the incongruity with the rest of his life. It is the same weakness of judgment which underlies the inadaptability of G. E. N. to his new environment when he is moved from one ward to another and causes him to call his new attendants by the names of those who cared for him on the ward from which he had just been moved. It is the weakness which underlies the inadaptability to environment of all early dements and such evidence of it as this patient showed in taking his tube home with him was sufficient to show that he had not yet fully recovered and he was brought back to the hospital. A few months later he began to eat solid food and after a few weeks he was discharged from the institution as cured.

The experimentation in this case brings out strikingly the stationary condition the patient had reached before I began the study of his case; and yet it shows the weakness of the processes of memory and judgment which remain as after-effects of the disease and furnish the chief factor in predisposing such a patient to another attack. The influence of changing conditions upon such persons is well shown and may be read in the variations in efficiency from week to week as represented in the curves of the experiments. These curves show neither a marked improvement nor deterioration but a mean variation much greater than that found in normal subjects. The experiments were modified exactly as in case O. O. L. except that in the word-memory test the words were not spelled out nor pronounced for the patient. Uniform conditions were maintained throughout the series of tests.

Case E. A. F., aged 26 years, single and a native of Ohio, but both parents are natives of Germany. The parents of this patient are living and in good health, and there is no history of insanity in the family. The onset of the disease in this case was characterized only by the appearance of delusions of a grandiose nature, accompanied by some hallucinations or illusions. These delusions persisted and formed the only striking feature of the disease during the time the patient remained at this institution. The symptoms correspond closely to those described as characterizing the paranoid form of dementia præcox[1] Judgment and memory were but slightly affected except as concerned the substance of the delusions. There were no physical symptoms and motor efficiency seemed normal. As is usual with such cases deterioration was very slow and no marked changes could be noted for the time during which the experimentation was carried on and but little deterioration could be seen in the two years he remained at this hospital. At the end of this time he was transferred to an institution in another part of the state and further observations were impossible. The tests used with this patient were the same as those for normal subjects and the conditions maintained were constant. The curves representing the results of this work bear out very closely Diefendorf's statement of the character and course of the disturbances of the psychic processes in this class of cases.

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[1] Defendorf, "Clinical Psychiatry" (Second Group of the Paranoid Form of Dementia Præcox), p. 260.

Case A. L. L., aged 27 years, single and a native of Sweden. The patient attended school in Sweden until he was fifteen years of age, at which time he came with his mother to this country. For the first three years after his arrival in this country he was employed in a factory in Binghamton, New York, and in this time he became addicted to the excessive use of beer and other alcoholic beverages. On leaving the factory at Binghamton he went to Chicago, where for about a year he was employed at various sorts of labor, most of the time in the packing houses. At the time of the Spanish-American War he enlisted in a regiment from Chicago and went to Cuba. Becoming sick he was discharged and returned to Chicago. He staid there but a short time and went to Pittsburgh, where he was employed for some months. From this point he started for the South and traveled aimlessly through several of the Southern States, remaining in one city only long enough to get money to go on and riding often from one place to another on freight trains. Finally he returned to the North and worked in several of the large cities, being much of the time in Pittsburgh and Chicago. Eventually he came to this city, where his father had been employed for many years in one of the large tailoring establishments. While here he staid at his father's house and put in his time learning his trade as a tailor. He was at this time about twenty-four years of age. While in this city his father would not allow him to become intoxicated and many quarrels resulted. After a few months here the son again went to Pittsburgh and from there to Chicago, where he remained but a short time. He then went to Seattle, Washington, and after staying for a few months with a brother there he traveled from place to place through Washington, Oregon, and California, just as he had traveled in the South a few years before.

While at San José (Cal.) he began to write peculiar letters to his father. In each letter he asked for considerable sums of money and the letters give other evidence of the development of delusions about this time. A short time afterward he wrote to his father from San Francisco stating that he was sick. A few weeks later he was taken to the Mendocino State Hospital[1] at Talmage, California. His committment papers stated that he had had syphilis, gonorrhea and chancre. When examined his pupil reflexes were normal and his knee reflexes were dull. On the second day he had a peculiar pseudo-cataleptic attack during which he could not be aroused. The following day he became very noisy and disturbed and this condition lasted for three days. His improvement from this time was uneventful and he was discharged as cured six weeks later.

On leaving the hospital he returned to this city and went to work in the tailoring establishment at which his father was employed. He did not work regularly however and was intoxicated much of the time. He had many delusions of persecution and of a grandiose nature. He believed that people were trying to murder him with gas, that he was to be imprisoned in the basement of his home, etc. Upon going to the tailoring shop one afternoon he demanded $15,000 for the work he had done that day and became very violent on being refused the money. Realizing that he was insane the foreman had him taken to the city prison for the night and two days later he was committed to this hospital. The interval between his discharge at the Mendocino State Hospital and his commitment to this hospital was forty-seven days.

Of the events connected with his committment and his removal to the hospital he can remember but little, exhibiting in this respect much the same condition as cases O. O. L. and A. B. H. When admitted to the hospital his judgment was fairly good and memory was good except as concerned the onset of the disease. During the first year his delusions gradually disappeared while memory and judgment deteriorated steadily. When I began the study of this case memory for recent events, judgment and apprehension were fair and the patient could tell a broken but fairly well connected story of his travels and his adventures in the various cities he had visited. The patient exhibited much the same condition as L. D. J., sitting usually in the same place, speaking to no one unless spoken to, and paying little or no attention to what was going on about him. His figure was rather slight and his shoulders were round and when he walked he stepped carefully as though afraid of making a sound that would disturb some one. His complexion was sallow, his hair very dark and his eyes were weak and shifty and were set well back under dark eyebrows. His forehead was low, his nose rather sharp and thin, and his mouth and chin betrayed a weak, vacillating disposition. His face never expressed either strong likes or dislikes, and, in general, it resembled very much the 'washed-out,' despondent expression so often seen in the later stages of paresis.

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[1] I am indebted to Dr. E. W. King, Medical Superintendent of the Mendocino Hospital for the Insane, for the facts concerning the commitment of A. L. L. to that hospital, his condition while there and his discharge from that institution.

With the progress of the disease memory, judgment and apprehension deteriorated rapidly and volition became so far disturbed that the patient found it impossible to give a continuous account of any series of events and could only answer simple questions. By questioning him carefully it could be shown that he had all the points necessary for an account of the event in question but the disturbance of volition was such that he could not combine his statements into a story. This is typical of the disturbances of volition in dementia præcox. In the meantime his emotional processes had so far deteriorated that nothing could change the despondent attitude which he maintained regardless of his surroundings. Thus he reached a terminal demented state quite similar to that seen in cases H. E. M., L. D. J. and in case G. E. N., which has not yet been described.

Case C. A. V., aged 24 years, single. The father and mother and several brothers and sisters of this patient are living and in good health and there is no history of alcoholism or insanity among any of the relatives. The patient had the usual diseases of childhood and has been rather sickly most of his life. He had a first attack when nine years of age and his physician regarded the present attack as a relapse from the effects of the first. The exact nature of this early attack cannot be ascertained. The patient received a common school education but had no opportunity to attend a high school. His occupation since leaving school has varied from time to time. Much of his time has been spent as a lineman in telephone, telegraph and traction construction, though he has been employed at times in various glass works, factories and foundries. The habits of the patient have always been good and there is no history of alcoholism or other excesses. A careful study has failed to reveal any sufficient cause for insanity. With a great deal of sickness as a predisposing factor it may have been due to worry and overwork, with insufficient care as to physical condition. Or this may be one of those cases in which a mind seems to go wrong without a sufficient external cause.

When the first symptoms of the present attack were noticed the patient was placed in a private sanatorium in this city. Two months later he was transferred to this institution for treatment and was examined at once by the physician in charge. His memory was found to be poor and he was unable to answer correctly many of the questions asked him concerning his home life and the events preceding the attack. Volition was somewhat disturbed and the emotional processes were affected. His mood was depressed and he showed no desire for the society or conversation of the other patients. He had no delusions or hallucinations at this time. He neglected his personal appearance and showed very little voluntary activity. For the first six months at the hospital he seems to have remained in this depressed mood with but little deterioration and his condition at the end of this time was but little changed. It was then that I began the study of the case.

About this time the deterioration became more progressive and continued for about four months, when the disease reached a crisis. Memory, judgment and orientation seemed completely lost. The patient became violent and was confined to a strong-room. Suddenly convulsive contractions of the muscles, particularly of the limbs, became a feature of the disease. Thus the patient would often, without warning, crush any object happening to be in his hand at that moment, as, for example, a tin cup from which he might be drinking. Fantastic hallucinations and illusions developed, followed by unsystematized delusions some of which were of a religous nature while others betrayed a perversion of the normal sexual instincts. During this time conversation was impossible and much of the time the patient muttered or shouted incoherent sentences. He often showed very typical examples of rambling thought, flight of ideas, etc. Thus he would talk for ten minutes at a time, the last word of one sentence suggesting the first word of the next, sometimes by similarity of sound, sometimes by association of meaning, but without the slightest association of ideas between the various sentences. This condition persisted for three weeks, at the end of which time the patient began to improve and the improvement in his condition for the four months following was as rapid as had been his deterioration. At the end of his fourteenth month in the hospital he was discharged as cured and left the institution in better mental condition than any discharged dementia præcox patient that I have ever seen.

The experiments used were the same as with normal subjects and the experiment curves portray strikingly the course of the deterioration and the recovery. It must be noted that for the ten weeks during which the patient's condition was most unfavorable no experimentation was possible, as shown in the break in the curves for this time. The last time the tests were performed was some weeks after the completion of the series proper, being on the day before the patient left the hospital. This was done to see how closely his work would approximate that of the normal subjects. In this last test some allowance must be made for the fact that in the weeks during which no experiments had been carried on the patient had lost some of the results of practice, as seen in the last tests of the normal subjects, and comparison should be made with the earlier tests of the normal subjects.

Case G. E. N., aged 23 years, single. Both parents are living and in good health and there is no history of insanity in the family. The father uses alcoholic liquors in moderation but rarely becomes intoxicated. The patient has used alcohol to some extent but has never been intoxicated.

The patient attended school and did creditable work but after finishing the fifth grade he left school and went to work as a messenger. Later he entered the employ of a pottery company as an unskilled laborer. He lost this position some time before his commitment to the hospital, and, at first, he was inclined to attribute his troubles to worry over this matter. The physician in charge of the case seemed to regard masturbation as the cause of the disease and to attribute the loss of his position to the onset of the disease and the consequent inability to perform his duties. When the patient was about nineteen years of age delusions began to develop. He believed that to eat certain kinds of fruit was committing a terrible crime and that he was committing murder in splitting kindling and in doing certain other kinds of work about his home. These delusions soon attracted the attention of his parents and friends and brought about his commitment to the hospital. During the first two years of his confinement at the hospital the deterioration had been rather rapid and had about run its course so that at the beginning of his third year at the institution, when I commenced the study of the case, he had reached the terminal stage in which deterioration is very slow. At this time the patient was poorly oriented and showed considerable disturbance of memory and judgment. He showed very little voluntary activity and the extent to which the emotional processes were affected was evidenced by frequent mirthless laughing at unexpected moments and without the slightest cause that could be assigned by the patient himself or detected by the most careful observation. This patient has never shown the despondency that characterizes so many of the early dements. The same mental condition has been maintained for the two years during which the case has been under observation and in the year covered by the experimental study no important change took place in any of the psychic processes. There were a few short periods of increased deterioration, always followed by a return to the former state. On these occasions the patient was always much disturbed and, in general, presented the same symptoms as did C. A. V. in his excited state. At such times he was usually talkative and profane and displayed flight of ideas and ecolalia. He also seemed to be completely disoriented and judgment and memory disappeared almost entirely for the time. These attacks rarely lasted long, though sometimes the patient did not completely recover from their effects for some weeks.

When last seen he had been at the institution for about five years, though he looked but little changed from the time when I first began to study his case three years before. He is of medium height, rather slight of figure and moves slowly and clumsily like an awkward boy of fifteen. Like A. L. L. he always walks with apparent care as though to avoid making a noise. His hair is brown and his eyebrows are rather thin; his eyes are blue and show the characteristic shifting from one object to another. His mouth lacks firmness and is often set in a meaningless grin. His expression is that of a boy, not a man, and there is about it always a look of fear like that of an animal habitually hunted. If spoken to suddenly or otherwise startled he will jerk sharply as though expecting to be struck and when addressed harshly he cowers like a whipped dog and his face takes on, for the moment, a pleading expression, but the next moment it is forgotten and he is again doing the thing for which he has just been reprimanded. That he has no cause for fear is shown in the fact that he has always received the best of treatment and is quite a favorite among the attendants and patients. It is merely one of the evidences of the disturbed emotional state resulting from the deterioration. Unlike A. L. L. and L.D. J. he does not sit quietly by himself but walks about the ward, talks to the other patients and sometimes pushes one of the padded weights used for polishing the floors. His hands are never still and he is continually picking at the buttons on his coat or playing with bits of trash which he picks up about the ward. His conversation too is characteristic. His voice is rather high-pitched, having never entirely changed and he always speaks in the undertone so typical of the early dement. Usually one must stand quite near him and listen closely in order to understand what he says. As already mentioned he exhibits ecolalic and ecopraxic tendencies and shows marked recurrence of certain words and expressions. Thus one morning when I talked to him everything was fourteen feet long or fourteen feet high or was in some way connected with the number fourteen.

Another feature of his conversation is his tendency to relate over and over certain adventures of his early life, describing such events in minutest detail. But regardless of the subject on which the conversation is started he is pretty sure to end it with a discussion of something to eat, his favorite topic being "soup beans." Thus his conversation, like his actions, is that of a boy. One observing him now, without having seen the earlier stages of the disease or knowing the history of the case, would judge him a case of arrested development rather than deterioration.

The experiments were simplified to adapt them to the condition of the patient and at times his condition was such that no experimentation was possible. This is shown in the curves by breaks at the places where the work could not be carried on. The curves in this case are of interest as showing the instability and great variation in the mental condition of a patient in the terminal or demented stage of dementia præcox.

Normal Subjects

Subject A, male, aged 23 years, single and a native of Ohio. At the time of the experimentation this subject was a senior in the university and a candidate for the B.A. degree. He had had some previous psychological training and, at the time this work was carried on, was engaged in experimental work in the psychological laboratory, as were also subjects B and C. With subject A the series of experiments was performed once each week for ten weeks. The conditions maintained were constant, the tests being always the same and always performed in the same order, at the same place and at the same hour and on the same day of the week. The experiments used were the ones already described and were performed exactly as described. The curves appear in the "comparison of normal subjects with dementia præcox patients" which is to follow and in which the curves of the normal subjects are contrasted with those of the patients.

Subject B, male, aged 22 years, single, a senior in the university and, like A, a candidate for the B.A. degree. This young man showed some well marked temperamental peculiarities and was, to a certain extent, lacking in the stability of character to be expected of a senior in the university and a man of his age. He did not have a definite object in view in his college training, as was shown by his selection of studies, both at this university and at the college which he had attended for two years before coming here. His intention was to prepare himself to teach, but what to teach he had very little idea. But withal, he was sufficiently normal for my purposes and, since he was working in the laboratory of experimental psychology, was found a very convenient subject for study since standard conditions were much easier to maintain there than with normals elsewhere. As with subject A the conditions maintained were standard throughout. This subject performed the series of tests once each week for nine weeks.

Subject C, female, about twenty or twenty-two years of age, single, and a sophomore at the university. She showed some peculiarities but they were not such as would affect the value of the results for comparison with those of the patients. As with the other normals all conditions of the work remained constant. The most striking feature of this subject's work was a higher degree of motor efficiency than was found in the other normals.

Subject D, male, aged 27 years, single, and a native of Ohio, but of German parents. This subject seemed normal in every way and in two years' acquaintance, a part of which time I was very intimately associated with him, I observed no abnormal or unusual characteristics. He is of good moral character and of average intelligence. In education and social relations he approximated as closely as possible the social class of the abnormal subjects. This subject attended school but a few years, going out early in life to make a living for himself. When 16 years of age he became a telegraph operator on one of the busiest railroads in this state and since that time he has been constantly employed in this capacity, the only exception being when he was employed as a locomotive fireman for a few months some years ago. During all the time he has been an operator he has worked twelve hours every night with occasional vacations of two or three days. However for a few months before this examination was made and during the time the experiments were carried out he worked in the day instead of the night. It will readily be seen then that his habits have always been very regular and, since his work brings with it a certain amount of exercise his physical condition is usually good. At the time of this series of tests he was in the best of health. He uses tobacco in moderation and alcoholic beverages sparingly, is never intoxicated and has never used drugs of any sort.

This subject being employed as he was, it was impossible to perform the tests in the daytime as was done with the other subjects and instead the work was carried on at night at his room. The conditions maintained were constant. The time of day for beginning the work and the amount of time required for its completion never varied; the place, the temperature and the light conditions were always the same. The experiments were performed exactly as with the other normal subjects. The only variable introduced was the difference in the degree of fatigue produced by his day's work on different days. Thus the curves of this subject represent the most accurate results it has been possible to obtain, and since the subject belongs to the same social class as the patients under study these curves are more valuable for comparison with those of the patients than those of the students at the university.

COMPARISON OF DEMENTIA PRÆCOX PATIENTS WITH NORMAL SUBJECTS

In beginning this comparison of the abnormal with the normal we shall first make a comparison of the various processes in the two classes of subjects as shown by the results in the experiments performed. These results have been presented as far as possible in curves which are so arranged that an upward direction of the curve indicates improvement or increased efficiency of the process under consideration while a descent in the curve marks decreased efficiency or deterioration. The numbers at the left of the figure indicate the value in time or per cent or such other unit as shall be stated for the given experiment. Since the tests were performed regularly at intervals of one week the curves correspond exactly in time, and the numbers at the top of the figure indicate the number of the week in which the results were obtained, as first, seventh, tenth week, etc. Since the character of the efficiency-measure varied so much in the different tests it was found impossible to arrange a uniform scale by which the results of all the experiments might be presented and the scale has been varied in whatever way seemed best adapted to the clear and concise presentation of the results obtained.

Taking first the processes of mental elaboration we shall begin with the results of the first three tests described, the addition test, the A-marking test and the card-sorting test. The results of each of these experiments have been reduced to curves which indicate the relative efficiency of the processes involved as measured by the time required to perform correctly a single act of perception and judgment. Thus in the addition experiment (see figure 1) the point marked by the curve at any given week indicates the time in seconds required for a single addition such as the one indicated here, the process of 'carrying' not being taken into consideration in this curve since by the methods used in this experiment it was impossible to detect the errors made in carrying the left-hand digit of the sum of one column to add it to the sum of the next column. In the A-marking curve (figure II) the time indicated is the time required for recognizing one A from the four other letters among which it occurs. In the card-sorting curve (figure III) it is the time required for judging whether a single card held in the subject's hand has red spots or black.

$$\begin{array}{r}7\\9\\\hline(1)6\end{array}$$

Studying first figure I, it will be seen that the first four cases, (cases H. E. M., L. D. J., O. O. L., and A. B. H.) are presented together, the same base line being used for all four of these curves. Likewise cases E. A. F. and A. L. L. are presented together as are also cases C. A. V. and G. E. N. The normal curves are all presented separately, each having its own base line. As stated above the number of seconds is marked at the left of the figure and the numbers of the weeks are noted at the top, making the value for any given week easily read. Thus in the first week H. E. M. took 4.35 secs. for a single addition

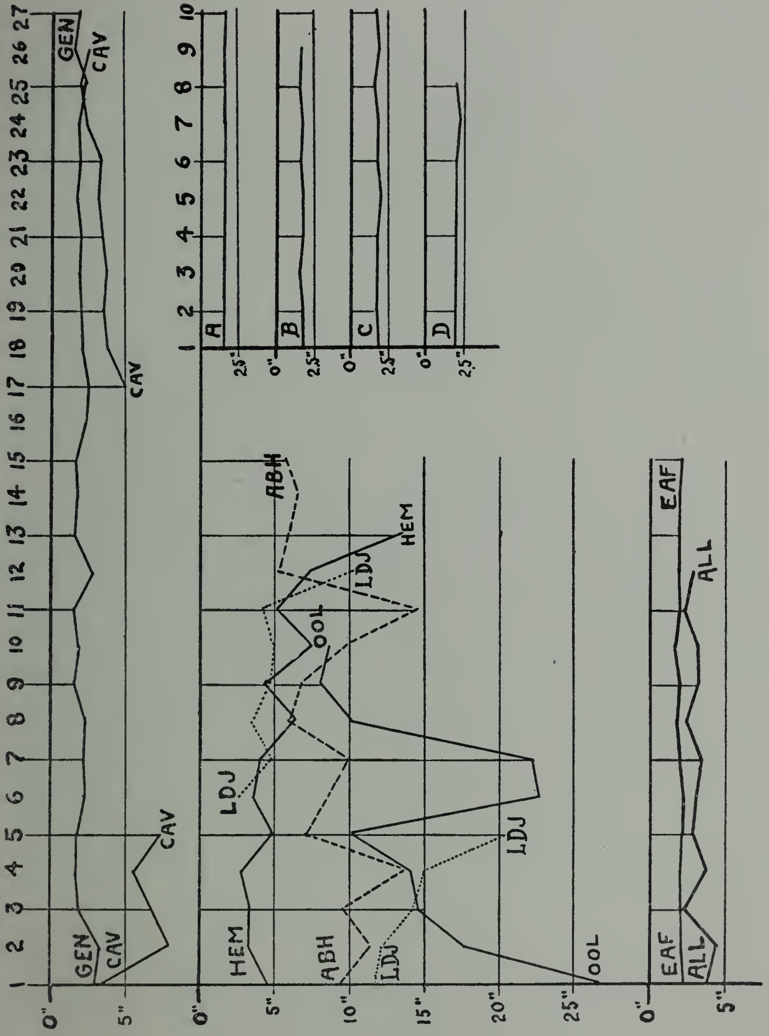

Fig. I. Addition Test.

while in the fourth week he used 2.75 secs. and in the thirteenth or last week of his series he took 13.02, these figures being taken from the table from which the curve was drawn.

The first striking difference to be noted between the curves of the normals and those of the patients is the irregularity of the latter as contrasted with the almost unbroken curves of the normal subjects which appear almost as straight lines, denoting a uniform degree of efficiency. This variation which characterizes the curves of the patients takes different forms in different cases depending upon the type and course of the disease. Thus in case H. E. M. we may note the downward tendency of the curve marking the deterioration of the processes involved. The abrupt descent toward the end of the curve marks the very rapid deterioration which ended in a state of such profound dementia that further experimentation was impossible. For case L. D. J. we find a curve which, beginning with a time of 11.77 secs. for a single operation descends very rapidly to a time of 20.41 secs. in the fifth week at which time it was found that the patient could no longer carry on the standard tests. Beginning then with the sixth week this subject used the single digit sums as used with cases O. O. L. and A. B. H. Thus a new curve for this patient begins with the sixth week at 2.72 secs. and continues to descend, though more slowly, with slight ascent in the eighth and eleventh weeks and ending with the abrupt descent to 10.17 secs. in the twelfth week, at which time the patient's condition was such that the tests were discontinued. Case O. O. L. was one of those patients who have remissions (see case history), and the ten weeks during which I experimented with him were marked by rapid improvement as demonstrated by the upward trend of the curve, beginning with 26.66 secs. in the first week and ending with 8.3 secs. in the tenth. The descent of the curve in the sixth week and the long time required in the sixth and seventh weeks (22.5 and 22.0) may be regarded as characteristic of the unstable condition, the weakness of the mental processes which remains even after a patient seems to have recovered entirely and which probably forms the basis for another attack after such patients have left the hospital as cured. Case A. B. H. was quite similar to O. O. L., the patient having reached a stationary condition characterized only by a single delusion. But the enfeebled condition of the processes involved was such that they were easily affected by external stimuli and as a result the efficiency varied greatly, giving a very irregular curve with ascending trend due to the improvement with practice and the slow improvement in the condition of the patient.

Case E. A. F. presents a curve which approaches in regularity those of the normal subjects and in this respect confirms the observations of the leading psychiatrists in regard to patients of this class. This is even more strikingly demonstrated in some of the other curves to be presented for this case and it is clearly shown that the efficiency of the various mental processes of this patient approaches most closely that of normal subjects of the same social class. Cases A. L. L. and G. E. N. belong to another class, their curves presenting the same characteristics. Both of these patients are in the terminal stage of the disease in which deterioration is very slow or has ceased entirely. These patients are in their deteriorated condition, like those who have apparently recovered, but to a much greater degree, affected by the conditions about them such as the weather, their physical condition or any change in their surroundings, etc. This is nicely demonstrated in case G. E. N. whose curve shows 3.32 for the maximum time (second week) and 1.73 for the minimum time (in the ninth and twenty-fourth weeks) and presents a continual variation between these two limits. In case A. L. L. we have the same condition, the limits in this case being greater with 4.29 as the maximum (second week) and 1.65 as the minimum (third week). Such irregularity is typical of patients in the terminal stage of dementia præcox and the curves of cases A. L. L. and G. E. N. are typical curves of the mental efficiency of patients in this stage of the disease.

The curve of case C. A. V. is of especial interest since it presents experimental data concerning the course of the disease, the deterioration and the improvement in the condition of a patient who passed rapidly into a state of extreme dementia, in which he remained for a few weeks, and from which he recovered to such an extent that he was discharged from the hospital as cured (see case history). Thus for the first few weeks the course of the curve is downward to the point at which it is broken (fifth week), the point where the condition of the patient became such that he could no longer perform the experiments. The curve begins again however in the seventeenth week and continues unbroken to the twenty-fifth week. The last test was performed some weeks later. The course of the curve during the last few weeks is irregularly upward, showing not only the improvement, the increased efficiency, but the irregularity due to the persisting weakness as was noted in cases O. O. L. and A. B. H., the other two remittent cases.

The curves of the normal subjects A, B, C, and D require little or no discussion. They present the regularity which demonstrates the uniformity of the condition of the normal as contrasted with the variation and instability of the early dement.

Having compared the efficiency curves of the patients at the hospital with those of the normal subjects we now come to the study of the deterioration of the processes involved in this experiment or, in other words, to a consideration of the kind of errors made. Upon a careful inspection of the record sheets it was found that over 75 per cent of the errors made by the patients were of a sort that *might* be due to errors in transferring the first digit of the sum of one column of figures (if the sum had more than one digit) to add it to the sum of the next column to the left, the process commonly known as 'carrying.' But the results obtained from these records were not conclusive since it could not be proven from the sheets at hand that any of the errors were of this sort and further experimentation was required to demonstrate the increase in the number of errors in carrying in the early dement. The data necessary were obtained by again performing the experiment a great many times with the subjects used in the original tests. In this work no time record was kept and the subject was required to repeat aloud the numbers indicating the various steps in the addition as it was performed and these steps were recorded by the observer on a sheet similar to the one used by the subject, the two sheets being filed together. This made it possible to determine the source of each error made and in this way it was ascertained:

(1) That of the errors made in the addition a very large percentage were due to errors in carrying.

(2) That this percentage varied with different subjects, dependent apparently upon the degree of deterioration.

(3) That the percentage of errors of this sort in normal subjects was small, and, owing to the small total number of errors made, could hardly be determined, and

(4) That it varied in normal subjects, depending to some extent upon the proficiency of the subject in adding.

With some of the patients studied it was found that of the errors made a large percentage were errors in carrying, the highest being 93.8 per cent in case A. L. L. while with patients showing very little deterioration the per cent was very much lower, as, for instance, 33.3 per cent in case E. A. F. Thus we seem to find a direct relation between the degree of deterioration and the percent of errors in addition which are errors in carrying. The significance of this condition would seem to lie in the disturbance of the attention and particularly in the restriction of the attention field, the restriction of the scope of consciousness as deterioration progresses. Thus as the scope of consciousness is restricted the mind naturally grasps the concrete more readily than the abstract and, inasmuch as the figures of the next column are directly before the subject, they are concrete, while the digit to be carried from the column just added is abstract and consequently is lost from consciousness. That these figures to be carried remain in the borderland of the field of consciousness, that they are just beyond the limits of the scope of attention and are readily brought into the field, was demonstrated by case G. E. N., who presented a peculiar type of error in the process of carrying—a type of error very rarely found in normal subjects. This patient would fail to carry from one column to the next added, but instead would carry to the second and in a very few cases to the third column added. That this was done has been demonstrated a great many times with this patient, and such instances have been found with other patients, while one such error was observed with a normal subject, seeming to prove that the number to be carried remains outside the scope of consciousness at the moment when it should be added in, and is brought into consciousness later by some process whose workings and disturbances have not yet been analyzed. There is, it is true, a similarity between the production of errors of this kind and the retardation found in some of the forms of dementia præcox, as when we ask a question and receive an answer some minutes later (cf. case H. E. M. answering the greeting after delay, see pages 12–13). In simple retardation we probably have a volitional disturbance which allows the setting free of a counter-impulse to retard, suppress or overcome each impulse received. Thus in the volitional disturbances so commonly seen we usually have retardation of all processes to a greater or less extent. In the case under consideration however we have one process going on undisturbed while another process intimately associated with it is in some way retarded or completely suppressed. This retardation is in the process which brings into the field of consciousness the abstract thing (the figure in the mind) which was for the time excluded, apparently by the concrete (the figures on the paper). But normally the number to be carried remains within the scope of consciousness and while the addition of the figures on the paper is a continuous process, the carrying must take place while it is going on. We cannot then say that the presentation of the abstract is simply retarded by the occupation of the attention with the concrete. We can only say, these errors are the result of a combination of disturbances of volition and mental elaboration by which the scope of consciousness is so restricted that the field is occupied chiefly by the more concrete imagery, while the process which should keep the other ideas within the field or bring them in when in some way they fall outside, is somehow retarded or suppressed.

The next two tests may be considered together since both are tests of the time of recognition and the same principles apply to both tests. In figure II the curves of the A-marking experiment, the numbers represent the average number of seconds required for perceiving and judging one A among four other letters. In figure III the curves of the card-sorting test, the numbers represent the average time of perceiving and judging the color of one card or one-fortieth of the discrimination time for forty cards.

In the curves of these two experiments we find the same general differences between the normal and the abnormal that were found in the preceding experiment, since all are measures of judgment. We again find the irregularity, the deterioration and the improvement of the early dement contrasted with the uniformity of the normal subject. But we find some differences between these two and the preceding experiment and we must study each and compare the results. Beginning with the A-marking experiment we turn first to the curves of the normal subjects. We find that the curves of A, B and C, the three university students, lie very close together, are quite uniform and present a slight upward trend, denoting improvement with practice. The curve for D however begins much lower and though fairly uniform presents a marked ascent due to the influence of practice. Since this subject corresponds very closely in previous training and in social class and condition with the patients his curve may be regarded as the normal for that class and we may regard greater ascent in the curves of the patients as indicative of improvement and less ascent or descent as indicative of deterioration. Thus in curve H. E. M. we may trace the deterioration from the outset and note that at only one point does the curve reach the level from which it started. This point marks one of those brief periods often found in dementia præcox when the patient's condition seems to improve for a few days only to be followed by more rapid deterioration, as seen in the abrupt descent of the curve from this point and the termination a few weeks later indicating the point at which the patient was no longer able to carry out the experiment. In curve L. D. J. we find a paradox. As the deterioration progressed this patient (L. D. J.) became more and more automatic in his performance of some of the experiments. Thus with tests where time was the primary factor the curve ascends, seeming to denote improvement, while the ascent really marks the course of the deterioration as would be evident from a study of the number of errors made. In this case the number of errors made the first week was sixteen. The next week the patient took a great deal more time and made only eleven mistakes. This being before the period of rapid deterioration and the patient being in fairly good condition he was able to benefit by practice and, having become familiar with the routine of the experiment, reduced the number of errors to seven in the third week. The work for this week represents probably the best of the entire series and from this point the number of errors begins to increase, the maximum being thirty-five or a little over one-third of the entire number of letters to be marked, while the time on this occasion was only 100 seconds, which is below the average for the normal subjects. It is evident that it would require much more than a proportional increase in time to increase the number of letters marked out to one hundred or, in other words, to reduce the number of errors to zero and the correction for accuracy made in the calculations for this set of curves is not sufficient. With the other subjects (normal and patient), however, the number of omissions was so nearly uniform that very little error was introduced and it did not seem advisable to sacrifice so clear a presentation of the results of the other subjects in this experiment in order to adapt it to this case. It was found that no uniform method of presentation could be used which would show both accuracy and speed without involving the pesentation of considerable amounts of useless data, since the other cases did not present this paradox. In curve O. O. L. of this test we find as in the preceding test the ascent marking the improvement in the patient's condition. Curve A. B. H. likewise presents a uniform ascent comparing favorably with those of the normal subjects. Thus the abrupt rise in the first few weeks marks the time when the effect of practice was greatest and the uniform rise from the third week may be regarded as partly the result of practice and partly the result of the improvement in the mental condition. Case E. A. F. again presents a uniform curve with slight ascent, probably due to practice, again furnishing evidence of the very slow deterioration in patients of this class. Cases A. L. L. and G. E. N. present the extremely irregular, abruptly ascending and descending curves to be expected from patients in the terminal stage of the disease. Case C. A. V., like case L. D. J., presented a paradox in that part of the curve marking the deterioration and, although the number of mistakes made increased as the work became more automatic, the increase in speed was enough to produce an ascent in that part of the curve.

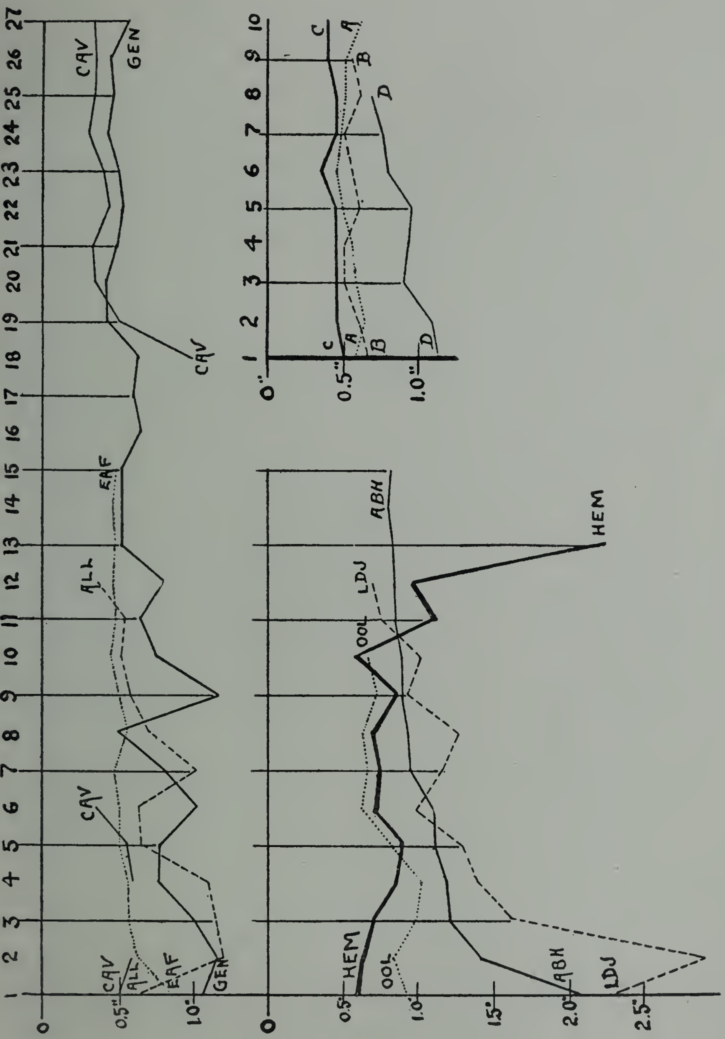

Fig. II. A Marking Test.

This automatism disappeared as the deterioration progressed further and did not reappear. Thus the second part of this curve represents the true value of the process measured and we see the ascent of the curve marking steady improvement to the end of the series of tests. The degree of efficiency maintained in the last four tests of this series was higher than that found in any of the normal subjects. The period of depression at the twentieth week is like many others we will find in patients who are recovering and is merely a sign of the persisting weakness.

In figure III we find the same characteristic features but with some exceptions. We find the normal curves much more irregular than in figure II and this has been in part accounted for in the various cases. Thus in curve A we find the time required for judgment remains as 0 for five weeks indicating that during that time he dealt the cards according to color (using perception and judgment) in the same length of time that he would deal them without regard to color (without the use of these processes). The same condition was found at times in nearly all the cases studied, though in only two cases was it particularly noticeable, these being cases O. O. L. and A. B. H. The curves of the other three normal subjects show first a period of descent marking the period in which the simple motor action of dealing the cards face downward increased in speed more rapidly than the dealing with perception and judgment. And when the limit of motor efficiency had been reached the effect of practice became more noticeable in the ascent of the curve. Among the abnormals we find as with the normals greater irregularity than in the curves of the A-marking experiment. Thus the curve of H. E. M. here shows more rapid and more marked deterioration than in the preceding test. But the two curves present enough similarity to show that the cause of their descent is the same and to prove conclusively that this cause is deterioration of the processes of perception and judgment. Curve L. D. J., instead of presenting the paradox seen in the preceding experiment, shows great irregularity with the general direction of the curve descending. The curves of cases O. O. L. and A. B. H., the two patients having remissions, show considerable irregularity but with the termination at a point show-

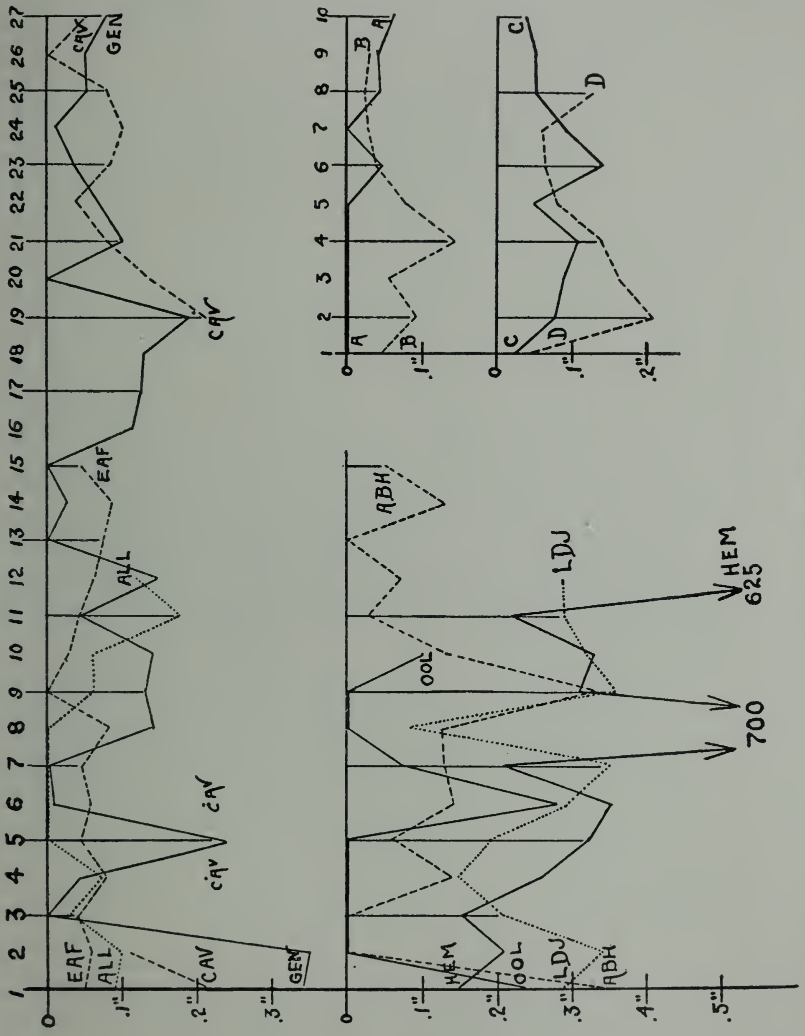

Fig. III. Card Sorting Test.

ing higher efficiency than at the beginning of the work. Curve E. A. F. is as usual the most regular presented by any of the patients. Curves A. L. L. and G. E. N. show the characteristic irregularity found in patients in the terminal stage, while curve C. A. V. shows the deterioration and later the improvement already demonstrated.

The question of most interest in connection with this experiment is that of an explanation for the 0 time of perception and judgment. It has been suggested that persons accustomed to playing cards a great deal would react quicker to the sight of a card face than to the sight of a back owing to the establishment of an acquired reflex through long practice. While this explanation might partially cover that of subject A it fails to explain that of case O. O. L. who had never played a game of cards, yet for six weeks of the ten covered by his series of tests dealt the cards with perception and judgment of color in the same or *less* time than that required for the simpler motor process of dealing the cards face downward. Both subject A and case O. O. L. fail to obtain any explanation by introspection. The most probable explanation is that the object or goal in sorting the cards according to color gives definiteness and precision to the act which is lacking when the cards are simply to be dealt in two piles.

The next experiments to be discussed are the tests of the accuracy of space perception as determined by the accuracy with which the subject judged the length of lines. The subjects compared the lengths of lines on cards, and they drew lines by comparison and by memory equal to a line seen. The curves for the last two tests (figures V and VI) show the error made in judging the lines used. Thus the processes measured, the method used, the basis of calculation and the scale of the curves used in the presentation of the results are the same for these two tests and they may well be considered together. For the first, however, a different method of presentation has been used (figure IV). In this experiment the subject was required to decide whether the various test lines used were longer, equal to or shorter than the standard line (100 mm.) and in this way the limits of accurate space perception were determined. In

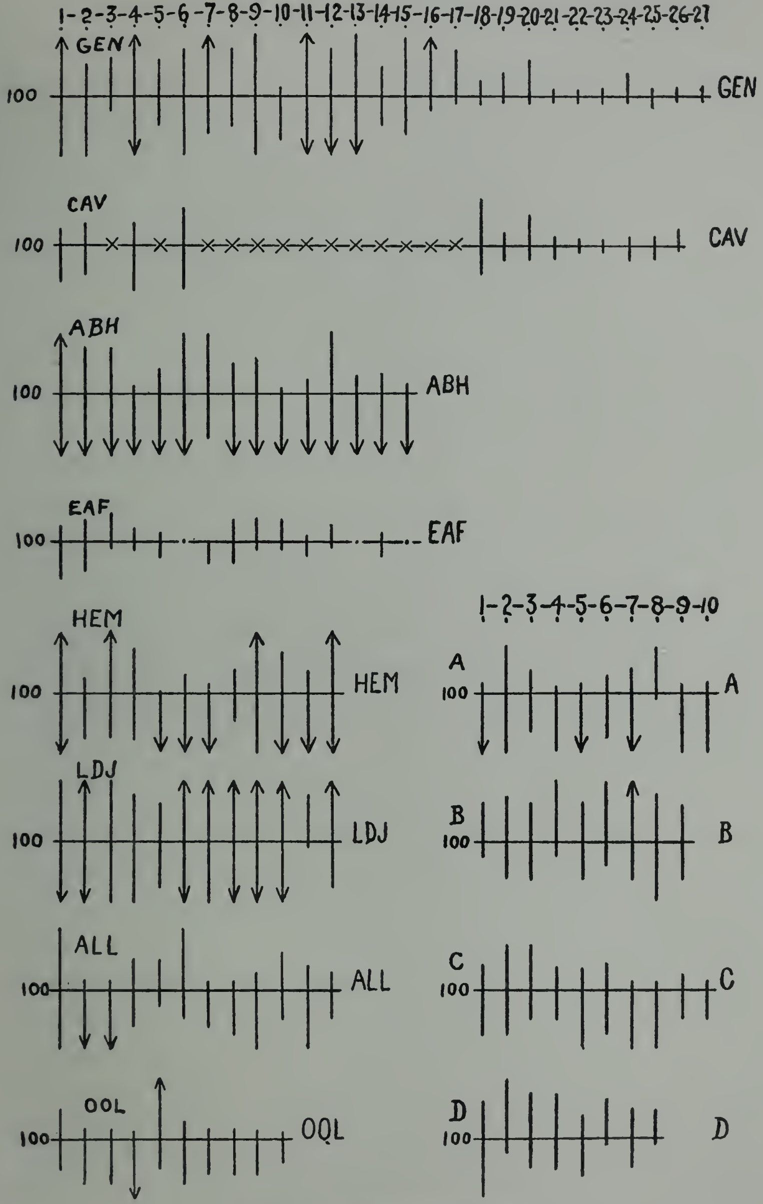

Fig. IV. Comparison of Lines.

this figure the base line marked 100 mm. represents the standard line, while the lines longer than 100 mm. are represented by the distance above the base line and the lines shorter than 100 mm. by the distance below the base line. The point marked (above and below) is the limit of accurate space perception, the point beyond which all lines are accurately judged. All the lines falling within these points are within the limits of accurate space perception and the subject is therefore uncertain whether they are longer, shorter or the same length and they are placed by guess so that while some of them may be correctly placed others are not. Thus in figure IV, case O. O. L., first week, the record line extends from the base line upward 4 mm., showing that a line 104 mm. in length was the shortest line that could be accurately perceived as longer than the standard line of 100 mm. For the same week the record line extends from the base line downward 4 mm., showing that a line 96 mm. in length was the longest line the subject could definitely recognize as being shorter than the standard line. Where the limit determined was at the normal, the fact is indicated by a dot on the base line for that week, as seen in case E. A. F. the sixth week, both limits were at the normal, every line being correctly judged, while in the following week the upper limit remained at the normal and the lower limit was at 97 mm., the 98 mm. and the 99 mm. lines being perceived as equal to the standard line. In some cases the limit or limits were not reached. This is shown by extending the record line to the 8 mm. point (the greatest limit absolutely determined in this series of experiments), and marking it at this point with an arrowhead showing that to record the limit it should be indefinitely prolonged. Thus in case H. E. M. for the first week neither limit was reached while in the third week the lower limit was at 6 mm. and the upper limit was not reached. When no experiment was performed for a given week a cross or X is markd on the base line for that week, as seen in the records of case C. A. V.

In comparing the records of the patients with those of the normals, as usual, the striking feature is the comparative regularity of the latter as contrasted with the former. But in this experiment we find some exceptions to the rule. Thus in cases

E. A. F., C. A. V., and G. E. N., and, to some extent, in case O. O. L. we have records which parallel those of the normal subjects. This is easily explained. Case E. A. F. whose space perception is seen to be more accurate than that of the normal subjects had been employed for a number of years in factories of various sorts where by constant practice great accuracy of space perception had been developed and it must be remembered that this patient showed very little deterioration at this time. Moreover the processes of space perception are among the first developed and among the last to be lost. We would therefore expect to find deterioration manifested more slowly here than in the other processes tested. To what extent this is true cannot be definitely determined from this series of tests since the limits used in the experiments were not wide enough. In many cases the actual limit of accuracy in space perception was not determined (see the arrows on the records).

In case O. O. L. we have a patient who was improving rapidly and who did not show very much deterioration at any time during this attack. Thus it is to be supposed that the processes of space perception were but slightly affected and the two tests in which the limits were not reached (fourth and fifth weeks) resulted rather from lack of control of the attention than from any disturbance of the processes of space perception proper. A comparison of this curve with the curves of the same patient on other tests for the same weeks (4 and 5) shows that the patient was somewhat disturbed about this time and that his tests for the fourth, fifth and sixth weeks showed lower efficiency than at any other time covered by the ten weeks of this record. In case C. A. V. we have a record quite typical of the course of the disease. In this patient as in E. A. F. the processes of space perception had been developed to a greater degree than in the normals studied, as shown by the record of the last eight weeks of the series. The first few weeks were marked by rapid deterioration, and this is shown in the curve. Thus for the first two weeks the limit is the same and is much greater than the limit of the last few weeks, which may be supposed to be normal or at least approximately normal. For the third and fifth weeks there is no record as the experiment could not be performed on account of the patient's lack of control of attention. In the fourth and sixth weeks the experiment was carried through and the increased limits give evidence of the deterioration. Thus in this case we have slow deterioration of the processes of space perception corresponding with very rapid deterioration of the other processes, no record during the crisis of the disease owing to the impossibility of holding the patient's attention long enough to perform the test, and finally, improvement during the first weeks of experimentation after the crisis and an apparently normal record during the last six weeks of the series.

In case G. E. N. we have a patient probably quite efficient in space perception but now in the terminal stage of the disease and showing considerable deterioration of the other processes, the disturbance of the attention being particularly noticeable. As a result it was impossible to keep the patient's attention on the test during the early part of the series and the limits shown from week to week vary greatly. About the seventeenth week, however, considerable improvement is to be noted and during the latter part of the series the work done was very good, the limits determined being less than those of normal subjects observed. This is to be explained by the fact that about this time some other work was begun with this patient, giving him considerable practice in judging the lengths of lines and perhaps training him to hold his attention on the work in hand better than in the early part of the series of tests. This curve thus shows that though the processes of space perception deteriorate with the deterioration of the other mental processes they may be restored almost to their normal efficiency by careful training even after deterioration has progressed so far that improvement in the more delicate mental processes has become exceedingly difficult if not impossible.

In the next two figures (V and VI) we have the curves presenting the results of the two other tests of space perception. As already mentioned the class of work and the method of presentation are the same in both experiments and as they were always performed together we may take them up and discuss them that way. Figure V presents the curves for the test in which a

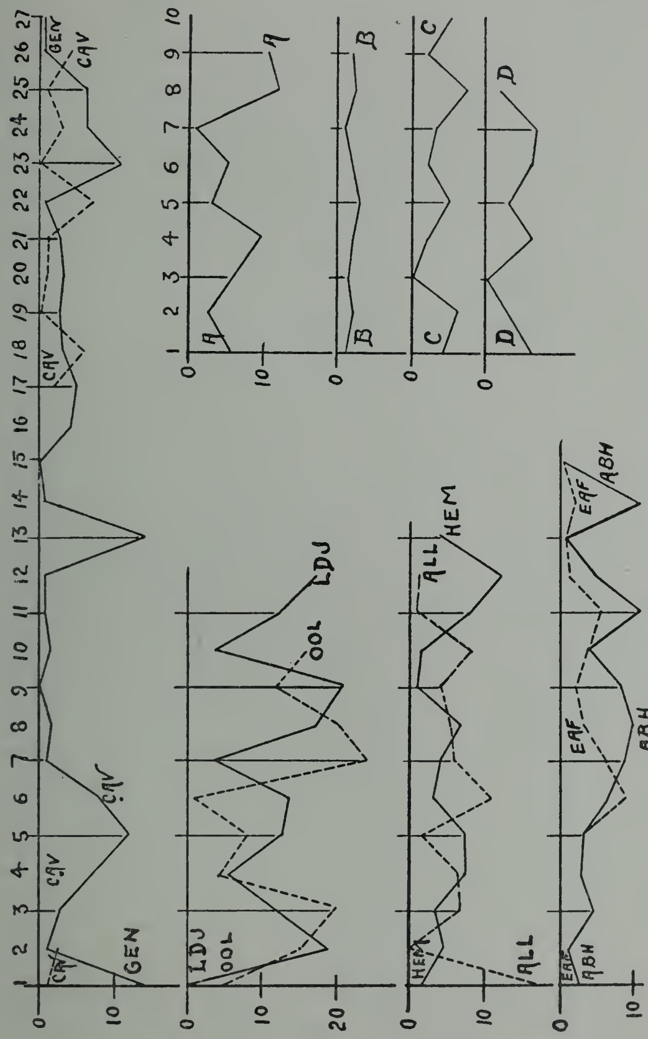

Fig. V. Drawing Lines by Comparison.

line was drawn equal to a standard line by comparison and the curve in each case marks the error in millimeters, the base line being above the curve so that ascent of the curve denotes fewer errors or increased efficiency and descent denotes more errors or decreased efficiency. The error value used in these curves is the difference in length (in millimeters) between the standard line and the line drawn by the subject as equal to it. Thus the line drawn being 97 mm. in length and the standard being 100 mm. the error would be 3 mm. and the curve would mark that point. Likewise a line drawn 104 mm. would have an error of 4 mm. Figure VI presents the results for the experiment in which a line was drawn equal to the standard line by memory and the same method of calculating the error was used in the preparation of the curves of this experiment. Each patient's record as shown in these two figures conforms with his record in the experiments already discussed. Again we find the uniformity of the normal subject and the irregularity, the deterioration and the improvement of the abnormal. But in all cases the deterioration and the improvement in the processes of space perception were slow and the curves do not show the continuous abrupt ascent or descent seen in some of the other figures. In curve H. E. M. of both figures we find great regularity and while in the comparison of lines (figure V) we find some evidence of deterioration, in the drawing of lines by memory (figure VI) we find little or none. In curve L. D. J. (both figures) the irregularity is more marked, as is also the descending trend of the curve, showing in both experiments more rapid deterioration of the processes involved than was presented by H. E. M. Cases O. O. L. and A. B. H. both show the same characteristics in these two experiments, as have been noted in the preceding tests, the curves being irregular with slightly ascending trend. Case E. A. F. as usual presents very regular curves which approach very closely those of the normal subjects, both in uniformity and in efficiency. Cases A. L. L. and G. E. N., the two patients who have reached the terminal stage, show as in the preceding experiments a great deal of irregularity with little or no evidence of further deterioration or improvement. Though we would naturally expect a lower degree of efficiency in these

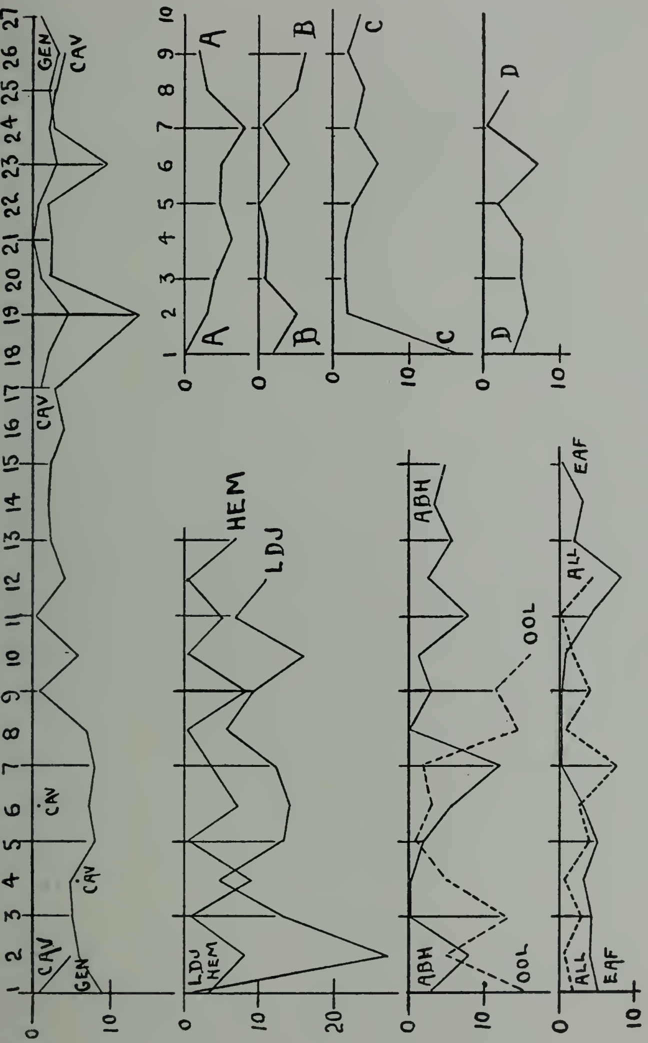

Fig. VI. Drawing Lines by Memory.

two patients, the explanation, like that in case E. A. F., lies probably in the nature of their previous occupation. It is characteristic that in the time covered by the work of these two patients there was no improvement. No other work was given which might act as training for this experiment and we may well contrast the work under these circumstances with that done by G. E. N. in the preceding experiment (figure IV), where special training was given during the latter part of the series. The conclusion to be drawn is that the deterioration has gone so far that the patient can no longer improve with the practice gained by repetition of the experiment at intervals of one week, while, if the interval be shorter, as in curve G. E. N., figure IV, some improvement may be noted. This conclusion is borne out by the conduct of patients in the terminal stages of dementia præcox, such patients making the same mistakes over and over and, though often corrected, seem unable to learn by experience or to improve with any ordinary amount of practice. It is possible that in the experiments any effect of practice may be counterbalanced and thus obscured by continual deterioration, slow though it may be. The effect of deterioration on ability to learn by practice has not been, in fact can not be determined to any satisfactory degree of accuracy.

The last experiments are the three tests of memory known as the 'letter-memory,' the 'picture-memory' and the 'word-memory' tests. Owing to the impossibility of reducing these results to a single curve which would show a complete and accurate determination of the efficiency of the processes involved in this group of experiments it seems best to present the results as in the original tables, compiled from the weekly reports.

The letter-memory test was performed by repeating a number of letters in an even tone of voice and at regular intervals and then requiring the patient to repeat the list as nearly as possible. A letter omitted was counted as one error, a letter not given but placed in the list by the subject, one error, while a letter changed from its position in the list was counted as one-half error. It will be remembered also that two lists of letters were used, the first list being repeated once, the second list twice, before the subject was required to attempt to repeat it. The records of the two lists are placed together in the presentation of the results of this experiment (Table II). In this table the record of each subject occupies a double column, the case initials being placed at the top of the column, and the number of the week being noted at the right and the left of the table. In the column of any subject for a given week are four figures. The upper left figure represents the number of letters read aloud to the subject in the first list and the upper right figure the number of errors he made in repeating those letters. The lower left figure shows the number of letters in the second list and the lower right, the number of errors made in repeating the letters in that list. Thus it may be remembered that the figures on the left always represent the number of letters given and the figures on the right the number of errors made. Taking the record of case A. B. H. for the first week it will be seen that the first list consisted of five letters (read once) and that in this list no errors were made. For the second list he was given ten letters (read to him twice) and in this list he made three and a half errors, leaving out two letters, placing one in the list which was not used and transposing one letter from its normal position.

In the comparison of the results of this test with normal subjects and with patients we can readily see the lowered efficiency of the memory of the patient as compared with the efficiency of the normal memory. This is shown in two ways in the table, the first being in the smaller number of letters used in the early dement and the second being the greater number of errors made by this class of subjects. Owing to the variation, even of the normal memory, dependent upon the physical condition of the subject and the many other factors influencing mental efficiency, it has been impossible to establish or determine the exact relation between lists of different numbers of letters or to determine the variation in efficiency necessary to give a constant per cent of error with a variable number of letters. Of course doubling the number of letters more than doubles the difficulty of the operation, and this must be borne in mind in comparing the work of normal with abnormal subjects and of the same subject at different times. Thus the

Table II. The Letter-Memory Experiment.

	HEM	LDJ	OOL	ABH	EAF	ALL	CAV	GEN	A	B	C	D	
1	10 8½ 10 3	10 7½ 10 4½	7 1 10 4	5 0 10 3½	7 3 7 1	7 1 10 5	10 7 10 6½	10 × 10 ×	10 7 10 3	10 4½ 10 4	10 4½ 10 3	7 × 10 10	1
2	10 4 10 3½	5 2½ 10 6	7 1½ 7 5	7 1 7 1½	7 0 10 5	7 2½ 7 4	10 5 10 3	5 2 7 6½	10 5 10 3	10 1½ 10 4½	10 4 10 3	10 3 10 3½	2
3	10 6½ 10 0	10 5½ 10 7	7 1½ 7 1	7 2 7 1	8 5 8 6	7 2½ 7 2½		7 4 10 3	10 2 10 3	10 3½ 10 0	10 3 10 2½	7 2 10 3	3
4	10 4 10 1	7 2½ 7 2	7 2½ 7 4½	7 4 7 3	7 6½ 7 1½	6 2½ 7 1	7 4 7 1	7 3½ 10 13	10 5 10 2	10 0 10 0	10 3½ 10 2½	10 4 10 4	4
5	10 5½ 10 12½	7 2½	7 4½ 7 3	7 1½ 7 ½	7 2 7 2	7 2½ 7 2½		7 6 7 5½	10 4 10 4	10 4 10 0	10 2 10 1	10 3½ 10 3	5
6	10 2 10 4	7 1½ 7 0	7 1 7 3	7 1 7 4	6 ½ 7 ½	6 0 7 1	7 1½	7 6½ 7 1	10 2 10 0	10 2 10 2	10 2½ 10 1½	10 4 10 5	6
7	10 4½ 10 4	7 2	6 2 7 1½	6 1 7 2	7 1½ 7 1	7 3 7 2		7 4 7 4	10 4½ 10 5	10 2 10 0	10 5 10 4	10 3 10 3	7
8	10 3 10 3	6 0 7 4	7 ½ 7 3	7 4½ 7 3	7 4 7 3	7 1½ 7 1½		7 ½ 7 4½	10 7½ 10 2	10 0 10 0	10 3 10 5½	10 3 10 5	8
9	10 0 10 4½	5 2 5 ½	5 0 7 3	7 2 7 5	7 ½ 8 4	7 4 7 1		7 6 7 ×	10 1½ 10 3	10 2 10 0	10 1 10 5		9

10	10 1 10 2	7 7 5 0	7 1 7 2	7 1 7 5	7 1 7 0	7 2 7 1		7 4½ 7 3	10 3 10 2½		10 5 10 7		10
11	7 4 10 3	5 ½ 6 ½		7 4 7 2	7 ½ 7 ½	7 2 7 1½		5 0 7 2½					11
12	10 3½ 10 9	7 2 7 7		7 1½ 7 4½	7 1 7 ½	7 3 7 3½		5 2 5 0					12
13				7 3 7 2	7 3 7 1			7 × 5 1½					13
14				7 5 7 3	7 ½ 7 2			7 3 7 5					14
15				7 2 7 5	7 2 7 2½			7 4 7 7					15
16								5 0 7 2½					16
17								7 1½ 7 6					17
18							7 1 7 ½	6 1 7 5					18
19							7 1½ 7 3	7 2 7 2					19

Table II. The Letter-Memory Experiment—Continued.

	HEM	LDJ	OOL	ABH	EAF	ALL	CAV	GEN	A	B	C	D	
20							7 2½ 7 1½	5 0 7 5					20
21							7 1 7 2	5 ½ 6 6					21
22							6 0 7 1	5 0 7 3					22
23							7 2 7 2	7 4 7 6					23
24							7 1½ 7 1½	7 2 7 4½					24
25							7 2 7 2½	7 5 7 5½					25
26							7 1 7 0	7 9 7 7					26
27								7 6½ 7 6					27

normal subject D in the first week of his series was given seven letters which he repeated correctly but when a second list of ten letters was read to him twice he made ten errors in repeating the list or one-half the total number of errors possible. Many similar instances will be found throughout the table of results. Another striking feature of this experiment is that normal subjects as a rule remember the letters better, repeat the list with fewer errors, if it be read to them twice than if it be read but once. With patients however we will find that in a great many cases, in the majority of cases perhaps, the subject makes fewer errors in repeating the list of letters if he has heard it but once than if he has heard it twice. This interesting paradox is to be explained by the difference in the method used by the two classes of subjects in trying to remember the letters spoken. The normal subject as a rule has a certain method which he uses in each case and the repetition serves to fix the letters in mind better by intensifying the imagery, be it visual, auditory or verbal, and by strengthening the associations already formed and setting up new ones if the method of associations be used in memorizing the letters. Thus the subject is able to retain more of the letters used if the list is repeated twice than if only once and each additional repetition tends to decrease the liability to error. With the patients we find a different state of affairs. Here, as a result of deterioration, we find marked disturbance of attention and of the other processes concerned in memory. Association is always more or less affected as are the fundamental processes concerned in the different kinds of imagery. As a result the more complex associations such as those usually formed in attempting to memorize abstract, meaningless letters are the first to be disturbed. The disturbance of the processes of imagery likewise comes on early, making any method of learning the letters very difficult. As a result of the disturbance of the processes of attention the patient does not confine himself to any one method of learning the letters but goes from one method to another and while he may form visual images of the letters when they are first repeated he is quite likely to use some other form of imagery or association when he hears the same letters again. Thus, instead of reinforcement, we are liable to find weakening and confusion due to the interference of the various methods with each other. This has been proven for normals by comparing the results in cases in which the subject used the same method throughout with the results where he was asked to use a number of different methods. Further evidence of the difference between the processes usually employed by normal subjects and those of the abnormals is to be found in the character of the mistakes made. With normals they are usually all of one class and are indicative of the method used, all letters substituted by the subject being, as a rule, in appearance, in sound, or in some other characteristic, similar to those for which each is substituted and showing that one method has been used throughout the list. With the patients we find errors of different sorts in the same list of letters, showing that two or more methods have been used on the same list. This is a typical example of the lack of thought control in dementia præcox and this disturbance of thought control plays a very important part in all of the disturbances of memory so characteristic of the early dement.

The other two experiments of this group are quite similar, (see the description of experiments), differing only in the character of the thing to be remembered and they may be considered together. The results of the 'word-memory test' are presented in Table III, and those for the 'picture-memory test' in Table IV. As in the preceding table, the weeks are numbered at the right and the left of the page and a subject's record throughout the series is read from top to bottom of the column indicated by the initials heretofore used to designate that subject. In each case record are four columns, the first being the time required to become familiar with the words or pictures on the cards used, the second being the time used in separating the original cards from the new ones, the third the number of cards used and the last the number of errors made. In studying this table it must be remembered that the number of errors includes both the number of original cards not picked and the number of new cards picked as original. The greatest number of errors possible would be made by failing to pick any of the original cards and at the same time picking all the new cards as

Table III. The Word-Memory Experiment.

	HEM	LDJ	OOL	ABH	EAF	ALL	CAV	GEN	A	B	C	D	
1	145 30 10 2	32 22 10 10	147 354 10 6	75 30 6 0	32 23 10 1	45 140 7 2	53 26 10 0	39 × 10×	54 18 10 0	21 26 10 0	19 20 10 2	30 24 10 0	1
2	75 25 10 1	20 29 10 6	86 140 3 0	56 52 7 1	30 23 10 0	18 22 7 0	26 42 10 5	23 × 10 0	17 22 10 0	11 19 10 0	23 20 10 1	50 55 12 3	2
3	20 15 10 2	27 × 10 8	46 398 7 0	43 58 7 1	27 25 10 3	20 14 7 1		35 × 10 0	17 30 10 1	16 18 10 4	19 17 10 2	35 28 10 1	3
4	43 22 10 0	20 26 10 7	22 236 5 1	72 56 7 3	9 16 7 1	21 27 7 0	29 35 10 5	45 × 10 1	20 23 10 0	13 16 10 2	15 16 10 2	36 30 10 0	4
5	130 65 10 2	12 35 5 4	40 140 5 0	48 55 7 0	9 16 7 1	8 22 7 4		× × 10 3	19 26 10 0	12 19 10 2	21 16 10 1	20 30 10 0	5
6	125 46 10 1	15 × 5 6	20 93 5 0	63 89 7 0	9 17 7 2	9 23 7 3	20 80 7 3	× × 10 1	20 22 10 0	18 19 10 1	22 17 10 1	23 34 10 2	6
7	100 58 10 0	10 40 7 8	28 118 5 0	90 60 7 1	9 17 7 2	11 15 7 0		× × 10 7	15 20 10 1	15 18 10 0	28 22 10 1	16 30 10 2	7
8	× × 10 1	7 115 7 8	35 × 5 0	80 63 7 0	7 20 7 0	15 12 7 1		45 × 10 1	12 19 10 0	14 20 10 0	21 16 10 4	16 50 10 4	8
9	75 28 10 0	9 11 5 5	30 147 5 1	65 55 7 0	9 57 7 3	10 17 7 1		70 165 10 0	13 19 10 1	16 18 10 0	22 25 10 1		9
10	20 80 10 2	9 60 5 2	15 131 5 1	83 107 7 2	8 49 7 4	5 10 7 2		40 × 5 4	14 20 10 0		19 16 10 4		10
11	25 40 10 2	23 50 5 4		103 65 7 0	11 22 7 2	7 13 7 1		20 142 5 ½					11
12	15 105 10 5	16 43 7 8		115 100 7 3	13 16 7 0	6 30 7 3		30 300 5 4					12
13				100 80 7 0	10 40 7 2			40 220 5 2					13
14				100 100 7 0	14 27 7 1			33 46 5 0					14
15				57 82 7 0	9 15 7 1			27 60 7 3					15
16								18 × 5 0					16
17								32 × 5 0					17
18							27 15 7 0	13 15 7 0					18
19							25 19 7 ½	40 × 7 0					19
20							22 48 7 2	24 × 5×					20
21							20 × 7 ½	27 × 7 3					21
22							26 16 7 0	96 300 7 0					22
23							16 35 7 0	67 230 5 1					23
24							14 20 7 ½	34 100 5 0					24
25							16 33 7 2	30 70 5 4					25
26							18 33 10 3	38 × 5 ½					26
27								40 210 5 0					27

Table IV. The Picture-Memory Experiment.

	HEM				LDJ				OOL				ABH				EAF			
1	65	57	10	1	32	23	10	4	212	126	10	2	42	41	10	0	25	16	10	0
2	57	30	10	0	57	45	10	12	49	38	5	0	37	41	7	1	22	×	10	1
3	48	35	10	1	22	×	10	9	50	100	7	0	32	24	7	0	20	20	10	0
4	50	35	10	1	×	×	5	2	30	87	7	3	36	85	7	2	16	12	7	1
5	45	×	10	5	72	×	5	0	100	190	5	0	60	23	7	0	10	18	7	0
6	×	×	10	1	7	50	5	4	80	90	7	0	53	63	7	2	12	20	8	0
7	47	55	10	1	7	×	7	7	50	24	8	0	65	50	7	0	16	15	7	0
8	120	50	10	0	10	54	7	8	55	55	7	0	38	33	7	0	9	18	7	½
9	45	46	10	0	12	9	5	5	48	95	7	0	27	60	7	0	15	31	9	0
10	48	89	10	2	8	50	5	4	38	58	7	0	68	140	9	2	12	19	7	0
11	53	37	10	0	12	48	5	6					62	52	7	0	12	17	7	0
12	15	46	10	1	15	67	5	6					53	45	7	0	12	17	7	1
13													60	40	7	0	8	10	7	0
14																	17	25	10	1
15													55	30	7	0	10	12	7	0
16																				
17																				
18																				
19																				
20																				
21																				
22																				
23																				
24																				
25																				
26																				
27																				

ALL				CAV				GEN				A				B				C				D				
32	30	7	0	42	22	10	0	63	91	10	10	27	16	10	1	12	14	10	0	44	19	10	1	45	40	10	0	1
20	34	7	5	17	27	10	1	35	98	10	2	17	16	10	0	8	13	10	1	33	20	10	1	33	72	10	0	2
15	23	7	2					×	×	10	10	30	25	10	1	14	25	10	1	20	22	10	1	32	50	10	0	3
19	43	8	2	38	32	10	0	25	×	10	2	18	28	10	0	11	19	10	0	16	18	10	2	50	38	10	0	4
14	16	7	1									20	23	10	0	12	23	10	1	14	23	10	0	30	45	10	0	5
10	22	7	6	×	×	5	0	×	×	10	2	20	×	10	1	12	17	10	2	28	25	10	2	30	31	10	0	6
14	23	9	0					×	×	10	0	17	29	10	0	15	22	10	1	35	22	10	0	57	35	10	0	7
18	19	7	1					35	×	10	0	15	22	10	2	15	22	10	2	29	20	10	0	40	21	10	0	8
7	12	7	1					60	×	10	0	15	18	10	0	15	17	10	2	29	18	10	2					9
19	16	7	5					30	50	3	0	17	18	10	0					24	17	10	1					10
6	15	7	3					70	75	5	0																	11
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				23	×	7	0	28	72	7	1																	21
				23	16	8	0	35	600	5	1																	22
				12	14	7	0	43	55	5	0																	23
				17	17	7	0	23	30	5	0																	24
				18	22	9	0	20	60	5	1																	25
				18	17	10	0	33	117	5	0																	26
								40	×	5	0																	27

original and would thus be equal to twice the number of original cards used. This explains why the number of errors (last column) is in some cases larger than the number of cards used for the test (third column).

In comparing the abnormal with the normal records on these two experiments we find the same points of difference that have already been noted in the preceding experiment. We find the number of cards used with the patients much smaller than with the normal subjects, and, as has been stated, it is impossible to determine the relation of one number of cards to another as regards the degree of efficiency necessary to perform the test without error. But in order to make a comparison of time and errors in the two classes of subjects it was necessary that the test be no more difficult for a patient than for a normal and therefore that the number of cards used with the patients be less than the standard number used with normals. In this way the number of cards used shows something of the great difference in efficiency between the normal and the dementia præcox patient. The next point to be noted is the number and the sorts of errors made by the two classes of subjects. A study of the table will show that the percentage of errors for the patients was much higher than for the normal subjects in spite of the fact that the test was made much easier for the former. Under the same conditions the normal subjects would show a much smaller percentage of error than they do. The character of the errors made as in the preceding test shows evidence of the characteristic disturbances of the memory processes.

The same disturbances are found as in the preceding experiment but they are evident in less degree as the work in this test is not so difficult and does not call into play some of the finer processes whose disturbances become more evident as the work becomes more difficult. The difference between the tests lies primarily in the difference in the thing to be remembered. In the preceding experiment it was a letter, an abstract thing, difficult to associate or to image. In the 'word-memory' and the 'picture-memory' tests we have something relatively concrete. The word or picture used is a symbol and involves more imagery than the letter. It is readily associated with something else to fix it in memory and lends itself readily to imagery of one sort or another. Moreover in these tests the subject is only required to decide when he sees a card whether or not it is one that he has seen before (during the test) while in the 'letter-memory' test there is no suggestion of the letter to be remembered. The subject must bring it out. The greater difficulty of reproducing is readily seen in normal persons who cannot recall a given name or word but who recognize it immediately when it is spoken. Thus these two experiments do not require such perfect imagery nor such a high degree of attention as the preceding one and do not show so markedly some of the disturbances evidenced in that experiment. The only other feature demanding attention is the time required for learning the cards given and for separating the new ones from among the original cards. In each case the subject was told to use only what time was necessary, and he knew that the time occupied was made a part of the record of the experiment. Under such conditions the difference in time between patients and normals is indeed significant. It will be found that the average time per card required by the patients is many times that of the normals, for, from the tables it is to be seen that the patients in many cases required more time for a smaller number of cards than was used by the normal subjects. This is significant of the disturbance of the attention which comes into play in this experiment, lengthening the time till, in some cases, it was found impossible to tell even approximately what amount of time was used. The particular form of disturbance exhibited was rambling thought or distractibility of the attention, the suggestion given by the picture or word on the card before the subject carrying him far afield. Thus a patient may sit for some time gazing fixedly at a card while instead of having his attention fixed on that card he is following a chain of rambling, dissociated ideas, connected perhaps by only a single word in each sentence or thought. Thus the end reached is likely to be an idea in no way connected with the one on the card and one which will tend to confuse the patient rather than to help him to fix the word or picture in memory. The long time required is significant and quite characteristic of dementia præcox.

When we consider the facts presented in the tables: when we see that the patient falls far below the normal in each of the factors here considered; i. e., in speed, in accuracy, and in the amount of the work done at a given time; we can, by correlating all these factors, obtain an approximate estimate of the efficiency of the memory processes in the two classes of subjects studied. It is only when we correlate these factors, forming an estimate of efficiency, that we can realize the extent of the disturbance of the memory processes in dementia præcox.

The next experiment is a simple measure of motor efficiency as determined by the rapidity of tapping on paper with a pencil. This record is presented in figure VII, in which the curves mark for the various weeks the actual number of dots or taps made in thirty seconds. An ascending curve means increase in speed and presumably higher efficiency though in one or two cases paradoxes are found which must be described and explained. Thus case L. D. J. shows in this experiment the same paradox noted in some of the preceding experiments. As the deterioration progressed the work became more automatic, seeming to show increased speed and increased efficiency which is really a decrease in motor efficiency, inasmuch as the work is automatic and not the result of voluntary control. The other curves show the same characteristics as in the preceding tests. Thus H. E. M. presents an irregular curve with descending trend but with some ascent, probably paradoxical as with L. D. J., just before the end of the curve at which time work became impossible due to the extreme deterioration. Cases O. O. L. and A. B. H. as in the preceding experiments present curves characteristic of the course of the disease and case E. A. F. presents an ascending curve showing much improvement with practice and furnishing evidence that the very slow deterioration in this case has not yet affected such simple motor processes. Curves A. L. L. and G. E. N. show nothing unexpected except the ascent in the latter part of curve G. E. N., and this has not yet been satisfactorily explained. In point of time it corresponds to the improvement in the judging of lines on cards and though there was some improvement about this time following a period of depression and accompanied by some improvement in physical condition it was hardly sufficient to account for such a marked improvement as is shown in these two experiments. It is probable that as work with some of the other patients was discontinued about this time and special work with this patient taken up as noted before, there was some improvement in motor efficiency due to the training (special work) used from this time and this may account for a part of the improvement. It must be remembered that the efficiency of this subject had been very low and that while the comparative increase was great the degree of efficiency as a result of this improvement was still low. The record for case C. A. V. is typical of the course of the disease, the direction of the curve for the first few weeks being steadily downward shows the decrease in motor efficiency with very rapid deterioration. The curve is broken for the weeks in which no work was done and, beginning again, ascends rapidly with the final improvement reaching a high degree of efficiency which is maintained during the last few weeks of the series. The curves of the normal subjects show no striking characteristics, presenting simply the uniformity and slight improvement with practice that we would be led to expect from the character of the work done on the other experiments.

The last to be discussed is the association test which has come into some prominence in the past few years. Its chief value in this work, as in much of the work that has been done with it by other investigators, has been in determining the habitual content of a subject's thought by discovering the ideas or lines of thought in which the subject presents the greatest facility of association. With normal subjects the associations have no particular significance. We find that in any line of thought, familiar to the subject, association is quick, accurate and easy, showing the normal command of his store of ideas. The answer words given (see the description of the experiment) were always logical associations with the original word, and the time was always short, varying from a fraction of a second to three or four seconds. Where words were used with which the subject was entirely unfamiliar no association word was given.

In the work done with this test on dementia præcox patients we find a number of results of particular significance. First,

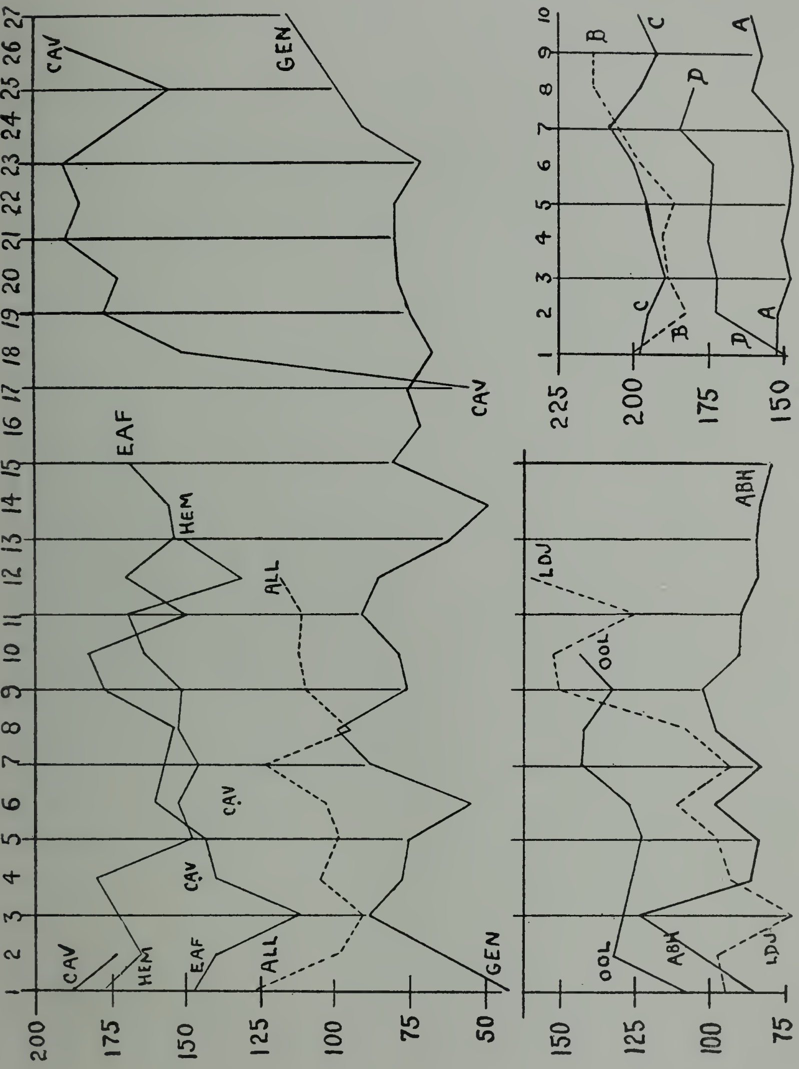

FIG. VII. MOTOR EFFICIENCY TEST.

and most important, are the indications of the content of thought, especially since they confirm the evidence from other sources. This test was used but little with case H. E. M. yet the few associations that were obtained showed the marked disturbance of the emotional processes and the predomiance of the sexual feelings in the content of thought. The same was true but to a less extent of cases A. L. L. and G. E. N. and at one time of C. A. V. Case E. A. F. presented as the chief content of thought his delusions regarding the German empire of which he believed he was to be the ruler. The associations in cases O. O. L. and A. B. H. and in the last few weeks of C. A. V. did not present any marked persistent idea nor did any single line of thought seem to occupy the mind to the exclusion of others. These patients showed apparently the same facilitity of association as the normals of the social class to which they belong. The weakness of the associative powers is shown however in these cases, as in the others studied, by a lengthening in the time required for a single association, or by the offering of answers which are not true associations with the test words used. At times associations which seemed to be illogical proved to be logical, but in such cases the answer was usually found to be, not the first association, but the last of a chain of several words or ideas, each associated with the following one and the preceding one as in flight of ideas or rambling thought. To obtain clinical records of such flights of ideas the subjects were given a single word and told to write in a column the words forming the chain of ideas following that word, the process being continuous for a given length of time. This time varied for the different subjects, according to the rapidity with which they formed associations. In this way many typical examples of flight of ideas were recorded and the lists showed plainly the effect of loss of the goal idea in the erratic and illogical tendency of association which results in the flight of ideas and the rambling thought so characteristic of the writings and conversation of the dementia præcox patient.

PSYCHOLOGY OF DEMENTIA PRÆCOX

What then are the mental marks of dementia præcox as shown by these experiments? We have seen the lengthening in time and the errors in carrying in the addition test, the missing of the A's in the second experiment, the misplacing of the cards in sorting, the inaccuracies of space perception, the disturbances of memory and association and the decrease in motor efficiency. On what do they depend?

To answer this question we must make a careful analysis of the causes underlying the errors and the retardation shown in the experiments and such an analysis will show that while there are several factors which vary in the different experiments according to the set of mental processes being tested, there is one which remains constant. It is the *loss of thought control.* Let us turn for a moment to the experiments and note its effect. That such a disturbance will have a marked effect on the time required for the various processes is evident for we can readily see that if a subject has not the normal control of thought and allows himself to wander mentally from the work in hand and then to return to it, the time during which his mind was not occupied with the work of the experiment is time lost. It is also probable that after the attention has strayed from the work it may return and repeat a part which had already been done, and of course the time required for this repetition would constitute a loss of time in addition to that during which the mind was not occupied with the work. That such a repetition may readily occur has been proven many times in the actions of the various patients on the wards and a good example of it is to be seen in the repeated greeting of H. E. M. as cited in the case history of that patient. Thus the increase in the time used in the various tests is a direct result of the loss of thought control, and the possible extent of its effect is nowhere better shown than in the lengthening in time in case H. E. M. (see Table I, case history). It will be remembered that in this case the loss of thought control was such that the subject would pause in the middle of an experiment to converse with his 'voices' and would then return to the work in hand. Toward the end of the series of tests with this subject the time on some of the experiments was lengthened to several times that in the first tests of the series. It was on account of the progressive decrease in thought control, lengthening the time used to a point where it could no longer be determined to a satisfactory degree of accuracy that the experimental work with this patient was eventually discontinued.

The effect of loss of thought control upon the character of the errors made in the different tests is even more significant. Thus in the addition test we find the errors in carrying growing directly out of the loss of thought control which allows the digit which is to be carried to escape from the field of consciousness by allowing the boundaries of that field of consciousness or thought to shift so that they no longer include that digit. These boundaries may again shift to include this digit and so give rise to the delayed carrying so often seen in G. E. N. In the A-marking experiment the error resulting from loss of thought control is the missing of the A's which are mixed among the other letters. Thus the subject may be marking out the A's in this list, his pencil point moving along the lines of letters designating the point up to which he has or supposes he has marked out all of the A's. If then, due to loss of thought control, his attention leaves the sheet of letters for a moment, the movement of the hand with the pencil point will continue automatically. When the subject's attention returns to the sheet it is to the place then designated by the pencil point, and one or more A's may be missed, having been passed by the moving point, during the time in which the attention was not upon the work. That this is the way in which the A's are omitted has been demonstrated introspectively and also by having subjects, both normal and abnormal, perform this test repeating the letters aloud. It may also be shown that the errors arise in this way by having the subject read aloud a long list of single digit numbers and add them instead of marking out some of them. The omission of numbers is shown by their not being read aloud and their not being included in the final sum. The principle of the omission is the same. In the card-sorting experiment the loss of thought control acts just as in the A-marking and the errors probably arise in the same way, the only difference being that instead of moving a pencil point along a line the subconscious act in this experiment is the dealing of the playing cards and when this goes on without attention the cards may or may not be properly placed. Thus the appearance of an error in the sorting of the cards from a lapse of attention in this experiment is not so certain to occur as one in the marking of the A's. The mechanism of the errors made, however, is the same.

In the tests of space perception a different set of processes comes into play but the attention is still of primary importance and in so far as it is affected space perception will be inaccurate. In the judging of lines on cards for example, due to momentary loss of attention to the work, one of the cards bearing a line 6 mm. longer than the standard may be picked as the same length as the standard line and though the upper limit of accurate space perception with the best attention of which the subject is capable may be 102 mm. and the lines between this and the one misplaced (that is 103, 104 and 105 mm.) may be properly placed. But the error due to the loss of thought control places the limit of accurate space perception as shown by this experiment at 108 mm. (the shortest line accurately judged as longer than the standard line).

The importance of attention to association and memory is too well known to require any discussion here, and such a disturbance of the thought control as has been shown in the preceding tests would naturally affect the accuracy of the work on the memory tests. The extent of this effect is readily seen in the error columns of the tables presenting the results of the three memory experiments. In the association tests the same underlying factor is again demonstrated in the illogical associate words given by the patient where only the first association was required; and the rambling train of thought shown in the associations when the patient was asked to write a list of associated words starting from a given word, is the homologue of the rambling thought in the patient's conversation which has been descibed as due to loss of the goal idea, and which is none other than the loss of thought control as we have demonstrated it in the preceding experiments. This is one of the typical features of dementia præcox conversation. It is found alike in C. A. V.'s ravings in the strong room and in G. E. N.'s descriptions of his boyhood excursions or of his experiences in the various wards of the hospital, and it is the same loss of thought control which prevents A. L. L. from giving a good narrative description of his life in America before his commitment to this hospital, while he can answer almost any question regarding the events of that period.

Many of the emotional disturbances described in the case histories of these patients grow out of and in part cause this disturbance of the attention. Interest and attention are one, and in the loss of thought control, the disturbed attention, as shown in the experiments, we have an idea of the patient's loss of interest in his surroundings and his consequent failure to adjust himself to them. It is because attention is not normal that L. D. J and A. L. L. sit for hours without taking any interest in the happenings on the ward or the actions and conversation of the patients about them, and it is from the same cause that G. E. N. when transferred from one ward to another tries to go to the dining-room by the same route as before and calls the attendants by the names of those on the wards from which he has come.

It is the loss of this control, that forms the basis for many of the volitional symptoms, making the patient negative in some cases and inefficient in others. He may be unclouded and be able to make good sensory judgments but this loss of the 'drive' or control of his mental processes makes him unable to evolve and follow out a rational procedure on the basis of these judgments and makes any complex judgment impossible. Thus when L. D. J. or H. E. M. is told to do a certain thing his sensory judgment is complete and correct but he lacks the control to enable him to execute the order. Instead, the control being disturbed, he does the opposite, or does nothing at all, and the result is known as negativism. He does not fail to do the thing directed on account of a complex judgment which shows him that the thing directed would be injurious to him. His judgment is correct but he has not the control necessary to enable him to act in accordance with that judgment.

Thus in conclusion we may say that of all the disturbances of which we find evidence in these experiments the most fundamental is the loss of thought control, since this control is the very fiber of the mind. The impairment and loss of this control of thought is the most important part of the progressive mental deterioration so characteristic of dementia præcox.

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An Experimental Study of Fatigue

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AN EXPERIMENTAL STUDY OF FATIGUE.

I. INTRODUCTION.

This paper will offer first a statement of the work that has been done in the particular field of mental fatigue. It will then attempt to show by certain modifications in the usual methods of experimentation how this subject of investigation can be materially enriched and simplified. The general method presented here has been used in other experimental work, but so far as our knowledge goes, has not been employed in any continuous mental tests, or to any extent in experiments planned to determine the changes in physiological processes incident to mental work. Few 'fatigue' experiments intended to obtain results by an objective record have planned definitely to check these results with a more or less complete introspective report of what went on during the process.

We shall leave a critical estimation of this combination method till we have shown its results, and proceed at once to a short statement of the work of some of the more prominent investigators in the field of intellectual activity and mental fatigue.

II. HISTORICAL.

Preliminary Remarks.—In writing an historical statement of the fatigue problem,[1] the selection of material for mention is peculiarly difficult. Fatigue, in its various meanings, is an universal phenomenon of life. To discuss the subject in its entirety simply means the consideration of every sort of vital activity as it approaches the ebb. Nor is it sufficient to stop there, since the consideration of this phase of activity instantly leads on to the problems involved in the phase that has preceded, the period from the beginning of any specific form of activity to the attainment of maximum speed or efficiency. Even then our task would be incomplete, for the great majority of writers, early and late, have made no clear distinction between a possible normal phase of fatigue and that which borders on, or is plainly within, the field of pathological phenomena.

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[1] The reader will understand that the following sketch is preliminary to the experimental portion of this paper and not a *review* of the entire field. As such it aims to summarize, not reviews of specific papers, but rather the phenomena found by investigators to accompany decrease in mental activity, giving in addition only such matter not immediately germane to this purpose as seems to be necessary for the clear understanding of the phenomena presented. From this point of view the historical part is intended to be more or less exhaustive and to present a fairly complete survey of the field at present experimentally covered.

Still, in the beginning, it is possible to use the more or less artificial division to which general biological science finds itself committed. For example, one may properly and without doing violence to the more specific aims of this paper, omit any extended reference to fatigue phenomena as they appear in the fields of zoölogy and comparative psychology; nor should we expect such a paper to be held responsible for a complete survey of investigations in the purely chemical and physiological phases of fatigue. Finally, anything more than a passing notice of those problems usually denominated muscular fatigue and glandular fatigue is omitted.

Muscle Fatigue.—In view of its historical importance, it seems advisable to begin with a short statement of the results of investigations in the field of muscle activity.

The main original source of information with respect to muscle activity has been the experimentation with the extirpated muscle begun in 1870 by the classic work of Kronecker (52). The field since then has been filled with innumerable workers and the principal facts with respect to the activity of the isolated muscle obtained. The later work of Mosso (66) on the muscles of man and the invention of the ergograph made possible a considerable field of investigation left untouched by the artificial conditions of the extirpation process. The work of Bergström (7) and Hall (38) has brought the experimental apparatus and criticism of the physiological conditions to their present high point of perfection.

The work curve has been in the focus of attention since the laws laid down by Kronecker (52). His first law is a statement of this curve of activity.

> The curve of fatigue of a muscle which contracts at regular intervals, and with equally strong induction shocks, is represented by a straight line.

The second law may be stated as follows:

> The height of the contractions diminishes the more rapidly the more rapid is the rhythm in which they are produced, and vice versa.

The animal on which his work was done was the frog, and the muscle used, the gastrocnemius. In the later work that has been done certain other facts have come out with reference to the activity of this muscle. The asymptotic character of the latter part of the curve was mentioned by Kronecker. His explanation referred this slowing of the decrease in the height of the contractions to the independent fatiguing of the 'contractile' and 'elastic' properties of the muscle, the latter fatiguing more rapidly and hence making the first part of the curve steeper than the end portion. The straight line character of the major portion of the curve persists whether the muscle be loaded continuously or be allowed total relief from the weight during the relaxation period. Hall (38) found recently that if the conditions were what he terms 'physiological,' the muscle of man, using the ergograph, could work indefinitely without signs of fatigue. By 'physiological' he means that it shall be compelled to carry a load proportional to the actual strength of the muscle, an amount to be determined empirically, and that it be allowed to rest from the strain of the weight during the period of relaxation. In earlier experiments, the muscle carried the weight back during relaxation and was therefore under constant strain.

The introduction of rest periods lengthens the time of the fatiguing process. Further, the muscle works longer if massaged or irrigated with defibrinated blood or some salt solution. Irrigation during the activity of the muscle does not change the form of the curve, but lengthens the period of contractility materially. After complete exhaustion the muscle requires a rest ranging from thirty minutes to two hours before it can again be stimulated to the maximum; in the case of the extirpated muscle this maximum cannot again be reached, but signs of renewed activity are present after rest. In the live frog, after seven days of rest a muscle thus exhausted does not fully recover histologically, masses of dead muscle tissue still remaining in the muscle.

The fatigue curve of warm-blooded animals is quite similar to that of cold-blooded ones. One important difference appears in the much slower single contractions in cold-blooded animals, i. e., the time of contraction is materially longer. Later experimentation shows that Kronecker and the earlier experimenters overlooked some important characteristics in their curves on the live animals (55). Both warm and cold-blooded animals show what is known as the 'Treppe,' which is a gradual rise in the height of the contractions for a short time after beginning work. This rise lasts sometimes as long as seven minutes in a muscle irrigated by the circulation of defibrinated blood or a salt solution. In some cases Mosso found this phenomenon in the muscle contractions of man, though a decrease or negative 'Treppe' was just as frequently present.

Hough (42) and Mosso both found that after the muscle of man had been fatigued to the extent that it would no longer respond to voluntary effort, it could still be made to contract under electrical stimulations. Hough reports that the opposite is also true, i. e., the muscle fatigued by the electric shock is still capable of responding to voluntary effort. He finds that in cases of mechanical stimulation, less work is done both in the height to which the weight is raised in the individual contractions and also in the number of contractions. Lombard (59) found that, if the muscle became fatigued under voluntary stimulation to such an extent that it no longer responded to the stimulus, the continuation of the effort, without altering the position of the muscle or otherwise aiding its recovery, would in time again produce contractions of the muscle. These contractions began after only one failure in some cases; and total exhaustion as indicated by this failure to raise the weight never lasted for more than a few attempts. When the voluntary contraction was alternated with one mechanically produced, the two fatigue curves were typically different and seemed independent of each other. The first showed the alternate exhaustion and recovery periods, while the second was the ordinary straight line curve.

After the first exhaustion, the extirpated muscle can be forced to do still further work by increasing the strength of the stimulus, by lightening the load, by massage, by irrigation, or simply by the introduction of a rest period. Pure water is also efficacious, but in less degree, as a circulating medium. By any of these methods, the result of the work never equals that of the earlier test. In living muscle, this is also substantially true. Mosso found that a rest of at least two hours was necessary before the total work of the first period could be duplicated. Voluntary straining prolongs the time needed for recovery, and nutritive and hygienic conditions exert important influences.

In the extirpated muscle, there is no experimental possibility of the muscle being renewed by anabolic changes other than the utilization of oxygen. Placing the contracting muscle in a vacuum shows, however, that the supply of oxygen is not a necessary factor. It was at first supposed that the muscle would continue to contract, if but one condition were observed, the removal of carbon dioxide. This has been shown to be only partly true, and three fatigue substances have been definitely isolated as final products of muscle activity. These three are sarcolactic acid, $C_3 H_6 O_3$, mono-potassium phosphate, $KH_2 PO_4$, and carbon dioxide, CO_2. The accumulation of any one of these in varying quantities will bring about the above described phenomena, and its injection into the perfectly rested muscle will produce all the results of the natural accumulation. It was by the use of these three substances that Lee (55) worked out with some degree of fullness the phenomenon of the 'Treppe.' He showed that the introduction of these substances in (a) minute quantities for a considerable length of time or (b) larger quantities for a shorter time, would produce not a lessening of the height of the contraction but an actual increase in the amount of work done. Presented to the working muscle in the reverse of these conditions (i. e., a minute quantity for a short time or a larger quantity for a longer period) the actual fatigue effects were produced at once. He concludes that their first effect on the working muscle is to increase its irritability, or to reduce the inertia and length of the 'warming-up' process, while their cumulative effect, if not removed by the circulation, is to reduce muscle irritability.

Speaking from the purely physiological standpoint, we find that the working muscle liberates heat in quantities far beyond the amounts that it gives off in the resting condition; its proteid compounds break down into simpler ones, three final compounds of which have been isolated; there is a greater demand for oxygen; for a more rapid circulation of the blood; and the metabolic processes in general are more active.

In some states that are apparently within the borders of the pathological, the muscles may be in a continuous state of contraction without any of these phenomena being present. In certain diseases, as muscularis dystrophia progressiva, and in special types of neurasthenia, the physiological and mechanical phenomena are modified in specific ways. Breukink (15) finds that in the first disease the height of the contractions is very much lessened, while the actual number of contractions that the subject can make is practically equal to that of the healthy muscle. In the latter disease, it frequently occurs that the subject is able to make a few contractions that equal those of the normal condition or are even greater, but they soon trail off into the tetanized condition or cease altogether.

In this connection it may be well to state something of the work that is being done in the field of the toxins in their relation to the fatigue problem. Dessy (23), Grandi, and Battelli in some experiments that were made on the suprarenal capsules early succeeded in isolating a substance that they called adrenalin. This substance when injected into the blood of a highly fatigued animal produced in him all the phenomena of a rested animal. They suggested that as a result of such experiments they had shown that there is a secretion produced in the activity of these glands that is antitoxic in its effects and acts by removing the fatigue substances from the blood. So also Weichardt (102) has quite recently claimed to have isolated a toxin that destroys the usual phenomena of fatigue. Adrenalin has been shown in recent experiments to have merely a tonic effect. It is no longer referred to by the physiologist as having any antitoxic effects of the type assumed by these earlier investigators.

The locus of fatigue.—What has seemed the most tenable theory for the array of facts that we have set forth? The stages of explanation have passed in their historical development from the simple statement that the muscle used up oxygen and liberated carbon dioxide through the various possibilities suggested by the blood circulation and is finally showing signs of a toxic explanation of the phenomena. The most widely accepted view at the present time seems to be a statement of the facts in chemical terms. That the fatigue substances so far isolated tend when injected into the rested muscle through the circulation to produce all the phenomena that can be produced under the ordinary methods, goes far to substantiate the truth of such a statement. In this respect, the work of Lee (55) has put the final touches to such a theory. On the side of the chemist himself, the theory is not so easily accepted. All he can tell us is that certain chemicals do produce contradictory effects when given in varying proportions; but where the actual transition takes place and its *modus operandi* is yet unknown. Those who would turn to the neurologist find that there is no definite assistance from that quarter.

Without going into a discussion of the various theories it will be seen at once that there still remain several important points not definitely settled. Regarding the theory of an antitoxin, Lee says it seems more probable that the function of the suprarenals is one of supplying a general tonus to the system than of manufacturing any specific toxin. The chemical theory of fatigue seems at present best suited to explain the more elementary facts of fatigue. The question of the transition from a stimulating to a depressing effect in the action of a fatigue substance is still one for the physiological chemist to labor over. In this particular place it has seemed appropriate to ask what is the neural mechanism that enables this change in the mere quantity of a substance to produce such opposed effects? A second large group of questions arises when we ask what are adequate physiological stimuli for the nerves of the visceral and deep-lying parts.

Experimental efforts to find the locus of fatigue are quite contradictory. Santesson (78) finds that the muscle will still give very large contractions when the stimulus is applied directly to the substance of the muscle after it has apparently been entirely exhausted by similar stimulations through the nerves. On the basis of other experiments that show the high resistance to fatigue of the nerve substance itself, Santesson (79) concludes that his experiments show that the first indications of fatigue are due to the wearing out of the motor end-plates in the muscles. In some later experiments made to corroborate his first results he finds that with a change in the strength of the stimulating electric current the muscle is still active; this change, under certain conditions stated in the article, can be one of increase or one of decrease in the strength of the current,—Wedenski's (103) condition of parabiosis. He does not find that this new result interferes in any way with his earlier theoretical conclusions. Schenk (80), whose article led to the repetition of the work of Santesson, finds that the results are not conclusive and concludes from his own experiments that the nerves and nerve endings are still functionally active. What actually happens is the loss of conductivity in the muscle tissue directly around the motor end plaque, and when the muscle is stimulated directly it responds as usual because it is now *adequately* stimulated. Joteyko (47), in experiments to be mentioned later and in her own review of the experimental field, argues with Santesson for the location of decreased functional activity in the motor end plaque.[1]

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[1] Beside the results indicated above Breukink (15) found that, in the curves of certain psychopathic patients where they had been fatigued by the ergographic test, a rest of two minutes enabled them to go on with the test and to make contractions that were as high and in some instances even higher than the first series of the test, a phenomenon that he never finds occurring in tests made on well persons. In chorea the curve, where obtainable at all, is very irregular, combining characteristics of the voluntary curve with many that belong to involuntary curves and are out of the control of the patient. It will be extremely interesting and enlightening when such tests can be made with more knowledge of the causes of such diseases and the relations that exist between the muscles and nerves.

Investigations to detect fatiguability of the nerves have resulted in but little of a positive character. The experiments on direction of the electric current in the rested and the fatigued nerve show clear indications of a reversal of polarity, and the work of Brodie and Halliburton presents some indications of the presence of CO_2 as a product of neural activity. Later work exhibits the chemical phenomena in still greater detail, but attempts no answer to our specific question. Recent unpublished work of Dr. W. Koch indicates in extreme cases of exhaustion slight changes in sulphur oxidation in the brain as a whole. Waller (101) claims to have found the production of CO_2 during mental activity. In general, however, the nerves may be considered as presenting a resistance to the effects of fatigue far above that of any of the organs involved.

The work on the nerve cell to determine its coefficient of fatiguability was begun by Hodge (40), and up to a recent date has been based principally on such histological investigations. Hodge found that the nerve cell actually showed definite structural changes due to its functional activity. Some of these were a decrease in the actul size of the cell, a loss of stainable substance, loss of other protoplasmic substances causing a vacuolated condition of the cell, and a ragged and more sharply outlined contour. In recovery, he found that for sensory cells in the cat, a period of recuperation varying from seven to twenty-four hours is necessary for the complete return of the cell to its former shape and structure. Bethe working on the motor cell of the dog found practically the same series of changes after a considerable period of enforced activity on the part of the dog. The later investigations of Wedenski (103) and others show in greater detail the changes that occur in the active nerve cell, but do not enlighten us with respect to the locus of fatigue. The cell body has been assigned by several physiologists, notably Sherrington (87), two functions, one a trophic function and the other the power to act through the summation of stimuli, leaving, therefore, the directional function for nerve impulses to the synapse.

The investigations of Sherrington (68) probably constitute the most brilliant series of experiments on the whole field of reflexes and problems connected wtih the 'blocked' pathway in nervous conduction. Through a long series of tests, he brings out a number of facts that are important for the proper envisagement of the field of neural physiology and one or two points that are of special interest for the problem concerning us here, he initial locus of fatigue. He finds that in the 'spinal dog,' one with a complete experimental section of the cord in the cervical region, after a time, certain reflexes return, notably the scratching reflex; and that these can be elicited by proper cutaneous stimulation in the thoracic region and at certain other points. The portion of the experiment of special interest here is that after cutaneous stimulation for a time in one small area, the muscles active cease to contract. But if a second place of stimulation be selected but a slight distance removed from the first, the same reflex may be immediately elicited, showing that the muscle itself is not in an exhausted condition. From this and other experiments, too long for detailing now, Sherrington (87) also eliminates the possibility of mere sensory fatigue and concludes that the primary origin of this phenomenon of 'negative induction,' as he terms it (so-called muscular 'fatigue'), is at the synapse. To this point in the pathway of the nervous impulse Sherrington assigns the function of 'blocking' the impulse,—through the accumulation of chemical products resulting from neurone activity,—and the further function of 'shunting' the impulse to other pathways.

Joteyko (47), in a series of experiments on the gastrocnemius of a decapitated frog, blocked the pathway to one of the muscles beyond the synapses and continued the stimulation of the other muscle till exhaustion; upon opening the pathway to the first muscle she elicited the reflex with no signs of 'fatigue.' She concludes with Santesson (78) that the primary locus of this type of fatigue is the motor end plates in the muscles. Woodworth (106), by a stimulation in the medullic region, was able to produce the same fatigue phenomena that Sherrington (87) reports with the possible entire elimination of the synaptic centers. So whatever we may think with regard to the facts offered, the locus of muscular fatigue, and equally that of mental fatigue, is not yet settled by experimental, physiological neurology.

These few scattered statements must close our review of the phenomena of fatigue as they are connected with the less properly mental functions. This much seemed necessary in order to show somewhat more clearly where the problems that will meet us as psychologists seem to lie; and how referring the locus and control of this 'negative induction' phenomenon and 'intellectual fatigue' to the peripheral end of the 'reflex arc' will not constitute a wide departure from previous theory. Sherrington, while presenting the theory of its central origin, still notes the close analogy between the phenomena obtained by himself on the 'spinal dog' and the results of Lombard's experiments where Lombard (59) shows the recurring recovery of the muscle without intervening rest periods. Though Lee (55) in his review of the fatigue situation reaches the general conclusion just stated, he finds it far otherwise with the questions of mental fatigue. He quotes from a number of prominent psychologists to the effect that 'feelings of effort' doubtless have their origin in the periphery, but can find no statements regarding the 'sensations of fatigue' and their nature.

We may now pass to work that suggests more specifically than the foregoing the possibility of a psychological treatment of fatigue. This work attacks the problem of fatigue from quite a different angle. Because of this its importance for historical purposes is great, although from the point of view of the whole it marks rather the beginning stages of the psychology of fatigue than a period of permanent contribution. Unfortunately for both physiology and psychology the early attempts in the two fields are quite distinct. In general, this holds true till we reach the work of Mosso and again after we leave him to enter the second period. The one exception to this is Kraepelin, who drew his inspiration from personal contact with Mosso's work.

Mental Fatigue and its Bodily Expression.—Mosso (66) first called attention to the possibility of measuring intellectual fatigue through its effects on muscular ability. His results have been severely criticised by later writers, though partially corroborated. The method he used has been refined until the major number of the criticisms have been met, but as yet no definite correlations have been obtained. The work of Mosso is, however, the beginning of investigation when we come to seek for physiological and psychological correlations.

Using the ergograph, Mosso finds, with a moderate load, at the rate of one contraction per two seconds, that the human muscle with voluntary contractions gives a fatigue curve different from the straight line curve of the frog's muscle. Two types occur, one a convex curve, the other concave. By applying electrodes to the arm and causing artificial contraction a curve approximating a straight line is obtained.[1] These types seem to be due to individual differences in becoming conscious of the 'tired' feelings. In intellectual and emotional excitement and strain these types persist, but certain others appear with them. For example, in the subjects tested, those whose curves of fatigue were concave in form (i. e., rapid fatigue effects at first, then slower) apparently find their muscular energy much reduced by an hour's lecturing or by holding examinations. On the other hand, Professor Aducco, whose curve of fatigue was convex, invariably showed an increased muscular power immediately after lecturing and during any intellectual strain, with a tendency of the muscle-work curve to fall much below the normal an hour or so after the strain was past. From the experiment of Maggiora (60), the laws of exhaustion are found to be dependent upon the previously fatigued or rested condition of the muscles or the brain.

These are the specific results of the ergograph work mentioned here, the details of which are given in the articles mentioned. The remainder of Mosso's discussion on intellectual fatigue is given over to a summary of collected observations from men who have shown evidences of severe mental effort. Many general phenomena of mental fatigue are noted, practically all of which are stated in a more systematic form in Galton's article referred to below. (See p. 30.)

Thorndike (93) finds that there is no correlation between the ability of school children to make records on the dynamometer and the amount of work they have done in the school room. Binet (9) finds the reverse to be the case, confirming in his work that of Clavière (12), who found, where the children were taken in three groups (one group that had been working at the severer mental tasks such as Geometry, Latin, Greek, etc.; a second on less severe mental work, such as reading in Literature, Geography, History, and a third whose members were allowed to laugh, play, sing and otherwise amuse themselves in ways that were not too physically fatiguing) that they showed marked differences in the dynamometric tests. The first group gave without exception lower records after than before the mental work; the second group showed practically a standstill, some going above and others below their previous record; while the third group gave a consistently regular increase in their dynamometer test. Schuyten (81) finds that the dynamometer gives no certain means of testing the fatigue of school children; they regularly give the better tests first, whether these tests be taken in the morning or afternoon. Smedley (89) considers the dynamometer fairly accurate in determining the intellectual endowment of school children. Dresslar (28) found in his tapping tests that there was a steady increase in each hour of work. He thinks that intellectual activity tends to increase muscular alertness, and 'fatigue' is wholly muscular and produced by the 'strain.'

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[1] By straight line curve, is meant that the line connecting the summits of the successive contractions is a straight line running downward, finally to meet the horizontal line of no contraction.

Scripture (82) found that, where the subjects were required to press a special form of dynamometer a certain amount, then increase that amount twice, three and four times, a continuous series of such judgments generally gave increasing inaccuracy and increasing uncertainty. He says (p. 75):

> Using 'fatigue' to mean a decrease in functional activity as defined on p. 14 we can consider both progressive error and the mean variation in the preceding experiments to be phenomena of fatigue. Although the actual force exerted increased as the efforts were repeated, yet, since they were intended to be equal to the first ones (or to the preceding ones), they became steadily less accurate. This increasing inaccuracy of judgment is properly a phenomenon of fatigue. Likewise the increasing uncertainty, as measured by the mean variation, is a constantly recurring phenomenon of fatigue.

Apparently the work done in this field need be pursued no further. There seems to be a singular lack of penetrating analysis with respect to just what the investigators working with the dynamometer are expecting, and no thorough-going criticism of the results. The criticisms of Müller (67) on the methods of Mosso are overlooked in the above tests, and no investigator reports any regular control series of tests on adults for the determination of practice effects, inurement and the like, such as were carried on by Bolton (10) with the ergograph. It is also curious that the work of Lombard (58) has been overlooked in the evaluation of these experiments. The work so far is entirely too haphazard to admit of any real estimate.

Relation of mental fatigue to automatic processes.—The next field where fatigue phenomena in connection with intellectual activity have been sought lies in the circulatory and respiratory processes. Only the main features of the work in this direction can be mentioned.

The work of Gley (38) marks the beginning of definite attempts to find a correlation between the progress of mental work and changes in the continuous automatic processes. In 1881 he made his first series of investigations, which he himself reviews in a monograph published in Paris in 1903. The statement he makes regarding his difficulty in determining the conditions of the problem and in getting unequivocal results is interesting and indicates great care in the conduct of the experiments.

The apparatus used was the cardiograph tambour adjusted to write the curve of the pulse from the internal carotid, a pneumograph for any changes in respiration and a Marey cardiograph for the heart curve itself. The apparatus was adjusted so that the hands and body of the subject were as free as possible when books or other objects were used in the conduct of the experiment for the mental tests. The same movements were carried out as nearly as possible during the period of comparative rest that were used in the control test. For the mental test, thirty of the experiments were made by reading Renouvier's "Traité de psychologie rationale;" twelve of them were made while working out problems in geometry, and ten of them while making a series of multiplications, e. g., 529 × 9, and similar problems written out beforehand. In one sense, he remarks, this last might be considered as a memory test.

Out of thirty tests whose results he gives, three showed no increase in the pulse rate, all the others showing an increase from one to six beats per minute; the period of work ranging from ten to sixty minutes in length. The longer periods of work were the reading tests. The amplitude of the curve of the single beat was much increased. This means that the arterial pressure is less, the walls of the arteries are under less tension as far as their own musculature is concerned. In the third place, the dicrotic changes of the pulse are accentuated and become double, a phenomenon that also indicates an increased elasticity of the artery due to the relaxed condition of its walls, and its consequent distention due to the influx of blood. This increase in amplitude and the change in the dicrotic are most prominent at the beginning of the work period and frequently drop back to the normal size in the rest periods, when the work is continued for fifteen minutes or longer.

Gley finds that the radial pulse shows precisely the opposite series of effects; there is, so far as he can determine, no change in the form of the curve as recorded by the direct beat of the heart; and he is unable to find any changes in the respiration curve.

He concludes that there is a 'dilatation vasculaire cerebrale primitive,' and the blood vessels of the head are filled by the pressure from without, caused by the vascular contraction of the peripheral blood vessels. Binet (9) does not find that the results of the experiments made by Gley and others support this view.

Binet (9) and Patrizi (70) find that the changes in radial pulse or in the pulse of the periphery in general are frequently caused by conditions that are not intellectual, and that they frequently occur independently of any change in the cerebral pulse. The conclusion that Mosso (66) first reached corresponded to the theory maintained by Gley, but was given up by him (66) in his later writings where he came to hold the view maintained by Binet, that the two vascular systems were independent.

Other results of this type of investigation relate to the differences that appear in short intense periods of work when compared with longer periods. Binet and Courtier found that in short periods of work, two minutes and less, the pulse rate increased rapidly and maintained this acceleration for some time after the close of the work. MacDougall (38) showed that this increase in the rate was not an immediate one but took place in a graded fashion to a point of maximum speed, then gradually decreased. Gley (38) in criticising these results, says that this change might be due to the emotional state of the first part of the period. In prolonged intellectual work the opposite is the case after the first acceleration. When two periods of considerable length are compared, one with comparative relaxation both of body and mind and the other with bodily inactivity alone, Vaschide (9) finds that the intellectual work tends to increase the normal slowing of the pulse. When the work is prolonged over a number of days the same result is noted, the average of the pulse rate on the rest days being 76.86, and on the work days 71.04.

Experiments by Binet and Vaschide on the changes in blood pressure show that the sphygmomanometer when used with a low pressure acts as a plethysmograph and with a certain high pressure, one equal to the constant pressure of the blood, acts as a manometer and registers the actual blood pressure. In using this last method they get results showing that in muscular and intellectual activity, there is an increase in the blood pressure, while the amplitude of the pulse as recorded by the plethysmograph remains unchanged. This result is obtained from the capillary circulation in the hand. With the low pressure apparatus the result is exactly the opposite, the amplitude is diminished very noticeably, indicating a vaso-constriction of the periphery.

Gley's conclusions, based on his own work and that of others, are as follows: Intellectual work tends to accelerate the heart but when long continued tends to sharpen the diurnal curve of decrease in rate; it augments the blood pressure in the peripheral arteries; and these peripheral changes cause a series of secondary changes in the pulse wave, as a sharpening in the angles of the wave, an increase in the number of the dicrotisms and a change in amplitude, and a decrease if measured by the plethysmograph; he also finds that the primitive dilation in the brain is a true phenomenon, and is attended by an increase in the volume of blood in the brain. These statements are corroborated by the results of others with the exception of the implication involved in the relationship that Gley finds to exist between the peripheral constriction and the cerebral dilation. Mosso in his later work, and Binet also, finds that as far as his present investigations will allow him to go, there is no relationship of a cause and effect kind existing between these phenomena. The argument that there is no causal relationship existing between the increase of the volume of blood in the brain and the intellectual effort, Gley disposes of with the remark that it is a common phenomenon that glandular activity always shows this reverse phenomenon in the beginning, and that the volumetric increase comes after the beginning of the mental strain is no argument that it is not a condition of that activity (p. 92, *op. cit.*).

Binet (9) in his own work and in reviewing the work of others, including Gley, finds that the objection to Gley's conclusion regarding the relation between the peripheral contraction and the cerebral dilation is well founded, and offers the method of determining the change in blood pressure as the most satisfactory one for determining the physiological effects of mental activity.

Pillsbury (72) found that there were indications of a relation between the attention waves as determined by the time when a gray ring was and was not visible on the Masson disk, and the Traube-Hering waves. The work of Zoneff and Meumann (62) on attention and respiration has shown that there is a distinct relation existing between the inception of mental effort and irregularities in the respiration curve. In addition there is a tendency to increase the number of respirations per minute in extreme mental work and a tendency to slow the rate in the more automatic activities. The work of Angell (2) and his collaborators showed the close relation between attention changes and the respiration curve prior to any other distinct recognition of the fact, and the later writers mentioned have corroborated their results.

The above authors find a fairly constant correlation existing between the pulse rate and the respiration curve. They accelerate together and show the major portion of their phenomena in complementary phases. There are some exceptions to this general rule, but when they occur they are regular in the individual showing the variation, i. e., if the pulse accelerates when the rate of respiration is slowed, this relation remains constant for the individual showing the phenomena. The later work shows that certain variations are probably due to the change from abdominal to thoracic breathing or the reverse, facts that were not brought out till the breathing curve was recorded by two pneumographs. The writers mentioned find that the number of respirations per minute increases and decreases with the changes of pulse rate mentioned above. The quickening shows itself in a shortening of the rest period after the expiration and by the decrease in the amplitude of the whole curve. This last is sometimes designated a change from deep to superficial breathing. Meumann finds that this last phenomenon is not in reality such a change, but is rather a change from the abdominal to the thoracic breathing or the reverse.

Notwithstanding the apparently distinct relationships that seem to exist between the varying degrees of mental activity and the physiological processes, there has appeared as yet no definite attempt to explain the relationships thus found to exist. We have taken considerable space to describe the results in this field because of this striking lack. It may be that some one will see the need for such an investigation and correlation and make a more ambitious attempt to test experimentally these changes and to explain them.

Summary.—The actual bearing on the fatigue problem of the investigations here summarized is slight. The assumption is made all through the series of experiments that the mental work has gone on with equal speed from the beginning, and with equal facility and freedom from errors. In the shorter periods of work measuring less than a minute, this is not so serious a defect, but in the longer periods of work some sort of correlation between the introspective account of the ease of working and the actual objective record of work done seems to be absolutely necessary if there is to be any definite determination of relationship. The main result of the work in this field so far seems to the writer to be one of promise merely. That the promise is a large one is admitted, but that it has so far any definite fulfilment cannot be maintained. Reference is made in this statement to the psychological import of the investigations. The one point that does seem to stand out in its psychological bearings is the relation between effort of the mental type that is made in the beginning of a piece of work, and the changes that accompany this effort in the breathing and pulse acceleration. Mental effort is shown by such relations to be bound up with the physiological expenditure of energy and the possibility of a distinct quantitative correlation becomes promising.

Reaction Time Tests.—Before passing immediately to the next field, i. e., of correlation between mental fatigue and sensory control, we may stop a moment to consider one type of experiment that seems to lie on the border line between the pure physical effects of fatigue and any sensory effects it may have. This border line problem is the reaction-time experiment.

Exner (32) found in 1873 that practice caused a decrease in reaction time, the opposing influence he called fatigue (Ermüdung). The phenomena of fatigue are much less striking than those for practice. The length of the reaction time increased or decreased with an increase or decrease in the intensity of the stimulus. A series of experiments on a man 77 years old whose reaction time was much longer than the normal showed that his nerves and muscles conducted as readily as a younger individual's and that the difference was, apparently, in the brain and nerve centers.

The first definite appearance of a fatigue problem is in the work of Professor Cattell. This work was done principally in the Leipzig laboratory and appeared first in a series of papers in the *Philosophische Studien*, Bd. iii and iv. The main portions of these articles are translated and appear in *Mind*.

In the first of the articles, Cattell (19) in discussing the time of cerebral operations and the conditions of the experiment refers to these latter as placing "the subject in an abnormal condition, especially as to fatigue, attention, and practice." In a paper reported in the same volume, he discusses at some length the effects of these three factors; special tests were made to determine their separate influence on the reaction time. Only the tests on fatigue will be mentioned here.

In thirty series of twenty-six reactions each, when the first of each in the thirty series were averaged together, the second in each and so on through the twenty-six, the first average was always the shortest, but the others show no regular increase in the times or 'increase in the mean variation.'

If, however, we take the averages given by Cattell and group them by fours we get the type of curve that Kraepelin (50) and his school call the 'work curve' in which fatigue is, according to their analysis, a prominent factor. The figures for these averages are for subject B, 143, 143, 147, 149, 153, 151, and 150 s, the last being the average of two reactions both of which are 150 s; for subject C, 148, 149 .75, 148, 150.5, 153.75, 147, and 147 s, the last two reactions 144 and 150 s. A more exhaustive series was then undertaken; 1950 reactions were made in a single day covering practically all the time from 8:30 a.m. to 11:00 p.m. in B's series and to 1:30 a.m. in C's series. The combinations of a series included five series of reactions, light, white surface, letter, association, and sound reactions; this combination was repeated six times. To quote:

> The first result to be noted from the table is the very slight effects of fatigue; in no case is the time lengthened more than a couple of hundredths of a second and the mean variation is but very little increased. We reach the very unexpected result[1] that the processes which are the most automatic (naming letters and C's simple reaction times) are the most affected by fatigue. The determinations made on the following day show that B had recovered from all fatigue. In the case of C, however, the brain substance concerned in the simple reaction seems to have been so far exhausted that his reaction time remained abnormally long for two days.

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[1] We shall hope to show in the sequel of this paper that such a result is, instead of being unexpected, exactly what might be looked for in the light of later investigations and their correlation with modern neurological theory.

With respect to daily increase in practice, Friedrich's (35) results agree with those of Donder (26), Exner (32), Cattell (19), V. Kries (51) and Auerbach. He finds in addition a practice effect which shows itself in a single day's experiments; the middle series usually shows a reaction time less than either the first or the last ones. The reaction test shows practice in both the simple and choice types, the latter being much more susceptible. Since only six to eighteen reactions were taken at a time, fatigue had no appreciable effect. Freidrich does not seem to consider the final falling off a fatigue effect.

Staude (91) remarks that fatigue of attention causes the speedy displacement of the apperceived idea by any other as well as by its related elements. This conception comes out in connection with a statement of the Wundtian doctrine of apperception.

Trautscholdt (94) finds that fatigue effects are most prominent in the case of those persons whose reaction times are fastest, and conversely. Only three subjects were used. He remarks that in the reaction to spoken words, in addition to the above, an uncertainty and perplexity of mind is felt, and that the mean variation becomes greater.

Kraepelin (50) finds in the reaction tests, after the immediate effect of anæsthetics has worn off, that in several instances, for ether and chloroform, there remains a feeling of fatigue that depends for its degree on the depth of the narcosis produced. With doses of alcohol up to 30 g. for those who are not regular users of it, a slight feeling of ease and an aroused condition appears, followed by a moderate feeling of fatigue. In the case of larger doses, up to 60 g., after six or eight minutes a feeling of intoxication appeared which gave rise to greater certainty, and greater clearness of perception, in reacting. This favorable influence gave way in the course of half an hour to an uncomfortable feeling of fatigue and stupidness and actual sleepiness. The effect of narcotics upon muscular and mental activity is at present an important field of experimentation. Rivers is perhaps the most active of recent investigators (76a).

When the reaction experiment is carried on continuously so that fatigue effects are allowed to accumulate, we have the fatigue experiment in its proper form once more. Two experiments of this sort are recorded, one by Patrizi and the other by Scripture. Patrizi (70) used the ordinary methods of determining the reaction time, with the exception that he allowed only intervals of two seconds to intervene between the reactions. He found a lengthening of the average time and an increase in the mean variation.

Scripture (82) in his experiments attempted to determine the rate at which the different factors used in the reaction fatigued. To do this the first series was made up of reactions to small flashes of light, where the observer sat in a dark room with both eyes open and relaxed. A second series was used with conditions as before except that one eye was bandaged to reduce convergence, while a third series was carried out with the subject looking constantly at the tube, one eye still bandaged, so that accommodation was a steady process. In the last test the room was lighted. Attention was present in all three cases, but the factors, as far as the eye was concerned, were considerably modified.

In the first record, after five minutes it was found that the reaction time had increased 60 per cent, and the time of holding down the key over 130 per cent, while the irregularity as shown by the mean variation was four times as great. With one eye bandaged, the increase in all three of these changes was distinctly less, while with one eye covered and a light in the room the change due to fatigue was scarcely apparent; there was an increase in the reaction time in the first five minutes of less than 5 per cent, an actual decrease in the mean variation, and only a six per cent increase in the period of inertia that preceded the reaction. The following conclusion may be quoted:

> The fatigue in reaction time increases with the complexity of the adjustments required for perceiving the stimulus. There is least fatigue when only an effort of attention is involved, more when the act of accommodation is added, and still more when the act of convergence is also added (p. 19).

Burnstein (5) found that there was a loss in the speed of the reaction time due to a 'general fatigue' from a day's work, of 15½ per cent in the simple reaction and of 17 per cent in the complex reactions. The morning record was taken at 8:30, and the afternoon record at 5:30. We may close this portion of the review by a quotation from Scripture's (82) article:

> There are three different phenomena denoted by the term 'fatigue,' (1) a chemical change in the constituent parts of the organism, (2) a diminution in functional activity, (3) a group of sensations.

Special and Cutaneous Senses.—We now pass to the more definitely sensory field where attempts have been made to determine the effects of mental work. We find an almost total lack of literature and attempts at experimentation, till we come to the subject of cutaneous sensibility. The special senses other than those of touch and the general cutaneous field are not, apparently, correlated with mental activity in the sense that the term 'fatigue effect' implies. As a result, there appear no definite tests on this topic. To be sure, numerous observations are recorded regarding the effect of continuous activity of one sense organ on its own power of reacting thereafter, and on the changes in fineness of discrimination. Results are especially clear in the realm of taste and smell, where the separate end-organs fatigue with considerable rapidity. In sight and hearing the fatigue effects arising in the epithelial end-organ itself do not appear so clearly. The fluctuations of attention when the sound or light observed are near the threshold of sight and audition, constitute in the minds of some observers a valid fatigue phenomenon; other investigators explain these changes in other ways. There is some tendency to call this 'fatigue of attention.'

Some isolated passages in this connection may be quoted, though no attempt need be made to evaluate them or find a relation to the specific field. The references are too sporadic and uncontrolled in most instances to constitute any more than naïve observations.

Berger (4) states that in his experiments to determine the variations in reaction times for stimuli of varying strengths, only attention of ordinary habitual concentration was used and the effects of practice and fatigue are not noticeable. In fact, they are reduced to such an extent that their effect on the reactions cannot be determined. He adds that the changes in attention are not the regular, steady increase or decrease that practice and fatigue ought to show.

Wolfe[1] finds that we have a species of tone memory or immediate recall in the case of tones, or, as Fechner stated it, an ability to retain after-images. This shows variations in degrees of clearness, and Wolfe concludes that these variations are due to changes in attention, even when the same amount of effort is put forth. This is fatigue of the organ of apperception (Apperceptionsorgan). This is, however, merely a suggestion. "A probable phenomenon of fluctuations of attention" is that "the periods will be shorter the simpler the object, and the greater the effort put forth." The actual fatigue as shown by the numerical variations of the two-hour tests in nowise corresponds to the feeling of fatigue. This, Wolfe remarks, may be due to the development of a temporary readiness to work (vorübergehende Fertigkeit) or skill (Einübung), which masks the real fatigue.

Lange[2] also refuses to call these fluctuations (when they occur during the continuation of the stimulus) instances of sensory fatigue, but assigns them to central causes, changes in the condition of the attention as it is directed to the stimulus (sinnliche Aufmerksamkeit). Münsterberg assigns them, however, to sensory sources.[3]

So far, therefore, the problems connected with the fluctuations of attention have not been brought into the realm of measurable fatigue, except as foot-notes and precautionary directions. The reaction time may with propriety be considered as connected with the sensory field; but in general the emphasis in the search for fatigue effects has been confined to the motor process. Beyond the work of Scripture (82) referred to above, and the work of Angell and Moore, (2) the writer knows of no attempt to assign proper fatigue values to the two phases of the sensory motor process. Baldwin's (3) work is merely a pioneer article on the analysis of these processes. Scripture says that in the first ten minutes of the continuous form of the reaction test the attention process shows little signs of fatigue. Angell and Moore find that attention goes with the part of the process that is least habitual. Where the subject shows considerable facility in one form of reaction, to direct him to turn his attention to the other portions of the process causes a rise in the reaction time and an increase in mean variation. Fatigue will also appear sooner when the subject is working under these new conditions than when working under the more habitual ones. Voss (99) found that in continuous additions there appeared a wave of effort which correlates quite easily with the attention waves found by Hylan and other investigators. We shall mention his work more in detail later.

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[1] *Philos. Stud.*, v. 3, 560 ff.

[2] *Philos. Stud.*, v. 4, p. 390 ff.

[3] Münsterberg, H.: *Beiträge zur Experimentellen Psychologie*, Heft 1, II, p. 69 f.

We must omit for the present any more specific reference to the work done on the changes in attention. It will again be referred to when the effects of mental work are discussed in connection with the experimental results of this paper. Once the fatigue phenomena are assigned to their proper place in memory, attention, perception, judgment, etc., we may then hope for a discussion of fatigue that will be able to emphasize the psychical side more than is at present possible.

Cutaneous Changes.—We have mentioned above that there is one field in which a definite correlation has been offered between sensory discriminations and purely mental fatigue, or the effects of mental work. The power to discriminate two points on the skin had previously received considerable experimental notice, but it remained for the staid German investigators to find that herein lay the means for preventing scholastic overwork.

In this field of cutaneous sensibility the work has been most completely carried out by Griesbach (38–1), Wagner (100), and Germann (36–1). Griesbach was the first in the field and found a distinct correlation between the work of the school room and the fineness of sensory discrimination on the skin. The principal test was the two-point discrimination test. Wagner and Vannod (97) also found a distinct relationship existing between the power to discriminate two points placed on the hand and mental fatigue. This capacity in their experiments is in inverse ratio to the amount of intellectual work done, being in the subjects tested fairly constant when the day was spent in resting or on holidays, but decreasing in fineness quite markedly on school days. They found that there was a definite ratio between fineness of discrimination and the kind of subject matter studied. According to their experiments, one subject of the school curriculum is decidedly different in its fatigue effects from its neighbor.

R. MacDougall (63) in a review of several articles on fatigue reports as follows on the work of Wagner and Griesbach. The method of Wagner is described.

> An area was selected upon the cheek and jaw of which the normal discrimination distance for two touch impressions was taken before school work began. The amount of fatigue was measured by the distance in discrimination acuteness which appeared after each successive hour of school work. This form of test has apparently proved decidedly satisfactory; it unites the simplicity of the physical method with a direct psychological fatigue factor. . . . The facts cited point to the conclusion that mental work of every kind is accompanied by general and not localized central fatigue, and it is questionable if new faculties can be appealed to. . . . All mental work involves fatigue of all and every part of the pupil's faculties, and there appears no absolute escape from it by variation of studies.

Schuyten (81) after testing all previous methods of determining the degrees of fatigue in school children finds the æsthesiometric test most reliable.

In Germany, some opposition developed toward this theory, particularly through experimentation done in the laboratory at Heidelberg. Bolton (10) working with Kraepelin found that a large number of factors arose in taking determinations as rapidly as these men did, all of which in his own experiments vitiated the results. The rise and continuance of after-images and tingling sensations were the more prominent of these factors. The relation of one portion of the skin to another immediately adjacent also prevented accuracy in the determinations.

Tümpel, after reviewing the work of Griesbach and Vannod, for theoretical reasons, finds their work wholly unscientific and unpsychological. Ritter (75), also for theoretical reasons, finds their results untenable.

Binet (9) and his collaborators in the schools of France find results that stand in close correspondence with the work of Griesbach. Using the æsthesiometer and algometer, distinct fatigue effects were found present in children of all ages after the work of the school day. These showed in the decreased ability to discriminate between two points and an increased sensitivity to the pressure of the algometer. Binet concludes by saying that the work of the psychologist is done and all that remains is the application of the method by the pedagogue.

The major portion of the work, however, that is hostile to the correlation found by these investigators has been done in America. Of this, the work of Leuba and Germann seems most conclusive. Leuba (56) went over the experiments of Griesbach and followed his method as closely as it could be determined from the articles published. He found there was no such ratio as Griesbach had found, in fact the opposite ratio was as frequently the case. He shows from the figures that Wagner published that the agreement between Wagner's and Griesbach's results is not nearly so perfect as Wagner had thought. Griesbach's results, so Leuba reports, show a rather regular and continuous decrease in sensibility lasting throughout the first hour, while Wagner's results show the decrease only at the end of the first hour. Leuba's criticisms on the methods used by these two experimenters are as follows: The rapidity with which the tests were made, six to ten persons in one area in ten minutes, or two or three persons in six different places in the same time, constitutes a feat that he considers as verging on the impossible; no preliminary tests were given; and in the tests made by Wagner, no precautions were taken to prevent errors due to the changes in temperature of the skin on coming into a warm room; the tests were taken in February and March.

Germann (36–1) in a series of carefully conducted tests on a single individual found that the number of errors in an equal number of morning and evening tests averaged 15.1 per cent ± 6.8 per cent in the morning series, and 12.6 per cent ± 9.1 per cent in the evening series; the percentage is based on the whole number of tests. He suggests as his conclusion regarding the validity of the methods employed by Griesbach, Vannod and Wagner, that the normal fluctuations found by Judd and Tawney (92)

> correspond quite closely with the amplitude of variation which Griesbach and more recently Wagner, ascribe to the influence of fatigue.

In regard to his own experiments he says:

> In at least one normal case the percentage of errors in cutaneous tactile discrimination bears no constant nor even relative correspondence to the mental fatigue experienced by the subject. I am convinced that in special cases the æsthesiometric method is absolutely inadequate for the determination of mental fatigue. Moreover, I strongly doubt its validity in any case.

From these results, one must still entertain strong doubts regarding the validity of the correlation found in a short series of experiments whose technical accuracy is so much in doubt. We are therefore left in uncertainty with respect to this attempt to correlate sensory acuteness of discrimination and mental fatigue. On the other hand, seeming to point to the possibility of such a relation, we find Kraepelin and his school insisting on a definite and positive correlation between the susceptibility to fatigue and its actual rise, and the capability of 'practice gain.' This last power is, by most investigators, definitely related to the sensory motor-arc and the establishment of a habitual pathway. But the preponderance of the evidence so far points to no such sensory specialization in the rise of fatigue, either as a 'diminution in functional activity,' or as 'a group of sensations.'

The work of Bettman (8) and others in determining the effect of one type of mental work on another uses functions too complex, adding, reading, memorizing, etc., to enable us to make a judgment on this point. Thorndike (93) found that fatigue was not transferable. On the other hand, practice is repeatedly found extending its influence beyond the specific stimulus-

and-response pathway actually used.[1] 'Negative' practice, or interference of training, may later be found to be definitely related to fatigue phenomena.

If the writer's personal reaction may be allowed at this point, it seems possible that under certain conditions a very distinct relationship may be found existing between cutaneous anæsthesia and analgesia on the one hand and specific forms of mental activity on the other. We believe, however, that experimental methods are not yet sufficiently developed to make this correlation.

Changes in the mental experience during the mental activity.—The approach to the final group of observations is fraught with considerable difficulty. There seems to be little or no continuity existing among experiments made to determine the introspective results of mental activity on the basis of the objective records of such mental activity. The major portion of the important work is based on subjects of school age and persons still in the lower grades of school work. The best controlled and most complete series of investigations comes from the laboratory at Heidelberg. This group of investigations is responsible for our knowledge of the characteristics of the work curve beyond the determination of the practice curve; of the effects of rest periods of varying lengths upon the succeeding work; and also for our knowledge respecting the effect of one type of work upon another in one or two special fields of activity. As was stated earlier, the positive side of mental effort has received a much fuller treatment, but not on the basis of what can be properly termed fatiguing conditions.

Two articles, historically earliest in this work, will be mentioned somewhat in detail, the first because of its actual value as an experimental ground-breaking method, and the second as indicating exceedingly well the complexity of the problems involved in fatigue experiments. Until recently the work of Sikorski (88) had received but the barest mention and it produced no direct reactions in work by other investigators till a considerable time after its appearance. Nevertheless, he succeeded in touching on more points of primary importance than most later investigators, and certainly his work ranks well with the majority of these more elaborate investigations. On account of its general nature, we shall, however, mention Galton's paper first.

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[1] The experiments of Ebert and Meumann, Fracker, Angell and Town et al. will suggest the line of argument in this respect.

Following out a suggestion that occurred to him in one of the discussions of the Anthropological Institute, Sir Francis Galton (36) sent out a list of questions to members of the Teacher's Guild.[1] Replies were obtained from 116 teachers and these, arranged under several heads, were reported at the meeting of the Institute held March 13, 1888. He states the object of the questions as, first, to determine if possible "the signs and effects of incipient fatigue in as measurable a form as possible;" second, to discover if the teachers themselves 'had ever broken down from overwork,' and obtain knowledge of cases that had come under their observation.[2]

Among the suggestions offered by teachers as to methods for determining incipient fatigue are (1) length of time during which neatness of execution can be sustained in performing a prolonged task; (2) promptness and sureness of memory in simple things; (3) common sense arithmetical problems; (4) reaction times. The writer summarizes in two conclusions. Mental fatigue seems to differ from muscular fatigue in causing worry and nervous excitement, thus preventing recuperation; and actual overwork is more apt to occur among those who work alone (but with class incentives) and are impelled to effort through eagerness to excel. In the discussion that followed the reading of this paper, cautions were offered respecting the value to be attached to such replies and the necessity of knowing the ability of the teacher replying. A further suggestion was offered that overwork was moie imminent where the number of branches studied was limited.

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[1] Rem. on Rep. by Teachers to Questions, Resp. M. F. *Jour. Anthro. Ins.*, 1888–89, pp. 157–168. The article also appeared in the *Revue Scientifique*, Jan. 26, 1889, pp. 98–103, and was reviewed in the *American Journal of Psychology*, v. 21, p. 340–1.

[2] In determining the condition of their pupils, Galton says the teachers depended on color of skin, dazed, jaded expression, nervousness, restlessness, twitchings of face, eyelids, etc. One teacher reported that she depended on

The experiments of Dr. Sikorski (88), made in 1879, were directed toward what are called psychomotor movements, meaning those movements that are voluntary, as talking, writing, etc. The theory is that fatigue (lassitude intellectuelle) will always express itself in some one of these voluntary movements, since they constitute the overt expression of ideas and thoughts.[1] Writing from dictation was selected as the best expression of the mental change. It was found that the errors made were distributed throughout a long or short dictation about equally, so no account was taken of the time in the actual tests made.[2]

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the color of the tips of the ears, if they were "white, flaccid, and drooping, she concluded the girls are thoroughly weary in mind," if 'relaxed but purplish,' she concluded that they are "tired not with study but from struggling with their nerves." Headaches, fainting, sleeplessness, irritability, and despondency are all assigned as effects of overwork.

In passing to special effects of fatigue, Galton suggests a test that all teachers could try and by that means bring out its value as a fatigue test. The teacher and a number of pupils with eyes closed take hands, forming a circle. On the table the teacher places a watch, with second-hand and records the time (viz., 'chain reaction') required for a squeeze from his left hand on the right of the pupil next to him to pass once or several times around the circle. He suggests that in a circle of 12 or 15 persons this will take perhaps one second. He says, "We should expect to find uniformity in successive experiments when the pupils are fresh; irregularity and prevalent delay when they are tired."

Numerous alterations of the special senses are reported, one case of actual color-blindness, loss of memory, mechanical processes in arithmetic become bewildering, failures to grasp meaning of simple objects and sentences in reading. One instance is reported where fatigue shows itself in a 'tendency to use long words.' Galton says, 'This strikes me as a very suggestive reply,' though he fails to add just why. Out of 116 teachers who replied to the personal questions, 23 had experienced serious breakdowns, 21 of whom had never entirely recovered from the effects. They express the difficulty of distinguishing between worry and over-work and their frequent reciprocal effects. "Sustained effort, vigorous inspection, quick decision—all are impossible."

[1] "Les mouvements psychomoteurs servent à l'expression d'actes qui s'exécutent avec discernment et forment un veritable langage des idées et des pensées de même que la mimique et les gestes forment le langage des sens.

[2] The mother tongue of the children was used, but no statement is made regarding the grade of difficulty, i. e., whether this remained in any degree constant throughout the experiments. The tests were made on eighteen groups of children, the series was repeated on two classes of three divisions each, making in all twenty-four tests. In all 40,000 letters were criticised for errors.

The averages in errors per 100 letters and per 100 pupils of the classes are as follows: first class, before school work 123.56, after, 156.68; second class, 121.48 and 145.27; third, 72.44 and 102.8; fourth, 66.47 and 94.20; fifth, 61.39 and 81.06; sixth, 45.70 and 80.05, making a general increase in the number of errors for the afternoon work of about 33 per cent.

In estimating the errors only those were counted that could not have been prevented by close attention and were not due to ignorance of the principle of writing.[1] Sikorski argues that those errors due to mere ignorance and inattention vary independently of the degree of fatigue and are outside the sphere of measurement.[2]

Psychic errors or omissions and substitutions are due to a failure on the part of the subject to distinguish between *small physiological differences;* the sounds in the above letters differ, but Sikorski analyzes them as differing but little in the position of the speech organs for pronunciation. This is confirmed by the tendency on the part of the pupils when fatigued to say the words in a low tone, or to increase the size of the writing "to recall clearly the movements of the hand and thus avoid graphic errors." Psychic errors are due to defective memory, hence lack of attention; they increase after the day's work 90 per cent. He says:

> This is explained by a psychical over-excitation under the influence of which the movements become more 'jerky,' larger, and more convulsive.

An error once made tends to be repeated. Three causes appear as productive of a lower grade of work, the decrease in ability to distinguish small psycho-physiological differences, weakening of the memory, and the appearance of over-mental excitation.[1]

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[1] "On tint seulement compte de celles que l'on nomme fautes involuntaires ou inevitable,—méprises—Les fautes proprement dites sont exclues du calcul,—" "méprises" thus are "en rapport avec l'exactitude du travail du mécanisme nervo-psychique pour un temps donné."

[2] After analyzing the whole process into three parts; the auditory or visual, the inner or cerebral 'reproduction a l'esprit,' and the final transformation into writing, he assigns to each its possibility of error, omitting those of the first process from consideration. 'Phonetic errors' are connected with the central process and changes in the acoustic composition of a word as 'ainsi' for 'ainse,' 'geurre' for 'guerre,' 'tonneurre' for 'tonnerre,' etc. 'Graphic errors' belong to the third phase, the representation of the word in writing, and are indicated by changes in the usual manner of writing, as lengthening or irregular tracing, crossing wrong letters for t's, omission or duplication of parts of letters. 'Psychic errors' are omission and repetition of words and replacing a word by its synonym. Finally a group is made whose character is indeterminate because of blots and erasures. In the above order the errors distribute themselves as follows in per cent per 100 errors: 73, 11, 6.5 and 11.95; m, c, n, v, l, o, k, and d are omitted most frequently in the order given, the first five furnishing 63 per cent of all omissions. r–l. p–b, d–n, t–n, b–v, and g–k are subject to frequent substitution. (The language used is Russian.)

Without attempting at this point to determine the value of his grouping of errors, we can see that by the method used by Dr. Sikorski, the increase in the number of errors at the close of a day of ordinary school work is plainly in evidence. The general increase being almost 33 per cent. Using dictation exercises, the marking out of certain letters, filling in blanks in a page of proof, etc., Ebbinghaus (29) found in 1897 that the number of errors increased from period to period. He also found that the amount of work done increased. Laser (54), in 1894, found that the power of production increased up to the fifth hour, and the number of errors increased up to the fourth hour. But the number of those that were correct also increased up to the fifth hour, so he finds the results equivocal. Burgerstein (16) finds practically the same thing appearing. The number of errors increases up to the close of the period. Miss Holmes (42), in working over Burgerstein's results finds that his method of counting the errors is open to criticism, and concludes from her own experiments that the "complex associations of computation are affected first, those of copying, less quickly," and that the reproductive attention disappears before the perceptual.

In the more complex tests, Richter (74), giving problems in Algebra and Greek verb forms obtained results similar to those of Sikorski and Laser. Höpfner (44) dictated sentences for long periods, and found that the number of errors rose during two-hour periods. The tendency in the type of errors seemed to be to drop back into the kind of errors that were habitual for that child. He does not state in this connection whether any precautions were taken to prevent a marked decline in interest on the part of the subjects used. Fredrich (35) used the same type of test and found that the work became generally poorer in the afternoon and at the close of any session of the school.

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[1] Both of these articles deserved the honor of beginning a series of researches along the lines laid down. Neither, however, produced any immediate result, and they only come into their rightful importance long after the work of Mosso. Bolton says, to Mosso is due the gratitude for recognizing and showing that fatigue is a general phenomenon, but this is as plainly the conclusion to be drawn from the material gathered by Galton or the earlier work of Carrieu (18). It is quite true that Mosso, whose work we have discussed, gave us an experimental method, though even here the work of Ebbinghaus and Kraepelin's pupils has shown the value of the method suggested by Sikorski.

Ebbinghaus (29) used three methods: the memory method used by Bolton in memory tests on Massachusetts school children, the computation method used by Burgerstein, and what he calls the "combination method." This last was a passage of prose, mutilated by leaving out words, syllables and letters, and the omissions indicated by dashes. The children were required to fill out these blanks so that the whole would make sense. He found by the first methods that the errors increased during the day. The tests were made at the beginning and close of each hour's work. In the third method the number of errors increased as did also the amount of blank spaces filled, thus the results are generally equivocal.

Thorndike's (93) experiments on adults and on school children show results that are somewhat at variance with the results above. In tests on adults before and after a ten-hour day of mental work, multiplications were performed by three subjects. Less accuracy was coupled with an increase of speed in two cases and an increase in both accuracy and speed in the records made by one subject. The results are not checked by any observations that indicate whether the subjects were all morning or afternoon workers, or were different in this respect. Ebbinghaus also found that the accuracy decreased along with an increase in the amount of work done in the 'combination method,' but no definite relation could be found. In an experiment using additions, Thorndike found that the best work was done before the day's work. His tests on school children in multiplications, spelling, and memorizing, show only a 3.9 per cent increase in the number of errors after the day's work, though they did 99.3 per cent as much work as those tested in the morning before the day's work.

Review.—Such work as this is as a whole open to Marsh's (60) criticism regarding the possibility of a daily rhythm and the large increase in inertia that seems to be present in the morning hours. Actual fatigue due to the work done itself is easily masked in any experiment or series of experiments by this phenomenon and by the effects of practice, as well as by the secondary phenomena of fatigue, as monotony, lack of interest, the 'glow' that comes at the close of the day's work, etc. In the second place, the whole attempt is too much in the gross to amount to more than a rough and ready method of determining the broader features of the more extreme states of mental inefficiency.

There seems to be a decided difference of opinion when the question of the amount of work done arises. In the cases cited above the general result seems to be in the more complex tests to find a gradual increase in the amount of work, and in the simpler ones to find no change or a slight decrease in the rate. The interpretation of this result has been as a rule to assign the increased rate to the effect of practice and inurement. In the second of Thorndike's experiments he finds that practice is plainly advantageous in judging the length of lines and is, in the test made, always enough to mask any effects of fatigue.

Characteristics of the work curve.—The following is in general the result of the work done in the laboratory at Heidelberg under the direction of Kraepelin (50). Beginning with the work of Oehrn (68) in 1889 and running through six volumes of the *Psychologische Arbeiten*, this group of investigators has analyzed the work curve for certain conditions of work into its essential elements. The main points that they have worked out may be briefly summarized. With the immense amount of material that must be reviewed in a few pages, it will be best to begin with Kraepelin's own summary of the work and mention in connection with it any further points from the experimental material that seem to illuminate the facts as he gives them.

The method pursued in these experiments is in its broad features the same. The addition of columns of single digits constitutes the chief fatiguing process. The length of time spent in any one series varied from thirty minutes to two hours. Two hours was the regular period for any continuous series where the purpose was to obtain a typical work curve including a record of both the character of the changes and the absolute amount of those changes. When the series contained rest periods it was lengthened by the length of the rest periods introduced. The records were divided into periods of five-minutes each, thus giving totals of numbers added for five minute intervals through the entire time of any experiment. Additions, reading aloud, reaction time tests, memorizing the Ebbinghaus nonsense syllables, and walking, constituted the fatiguing occupations. All of these would be used where, as in Bettman's work, an attempt was made to determine the effect of one kind of mental work on another, or the effect of physical fatigue on mental work. The precautionary conditions of the experiments are, where stated, entirely satisfactory.

The factors in the curve that are found to persist and are considered typical of the curve are: effects due to practice (Uebung), fatigue (Ermüdung); the immediate effects of each of these and their more lasting effects. The permanent effects of fatigue may, if the person be normal, be disregarded. After these more important factors, he finds in the curve secondary factors, such as overcoming a preliminary inertia (Anregung); a preliminary spurt (Antrieb), of two kinds, one lasting nearly a minute, and a second which usually covers the first five-minute period; a fall that occurs before the final maximum of the period is attained, due to the appearance in consciousness of the feeling that the work is not going as well as it has been (Ermüdungsantrieb); an alteration of spurts and declines in amount of work done (Willenspannung and Störungsantrieb); and the final spurt that occurs near the end of the series or where the subject feels that he is approaching the end of the test (Schlussantrieb). This last does not appear when the end is not expected. In connection with each of these there is the individual capability of practice (Uebungsfähigkeit); the relation it bears to the power of retaining practice effects (Uebungsfestigkeit); individual endurance in the single tests (Ermüdbarkeit); or when individual endurance is combined with the initial spurt, a certain preparedness for work (Arbeitsbereitschaft); Arbeitsbereitschaft combined with Anregung results in a characteristic habituation (Gewöhnung).

Effect of pauses.—In addition to this series of general phenomena, there appear numerous changes when pauses of varying lengths are introduced into the series. These pauses may be introduced at different time distances from the beginning of the series, or differently with respect to the varying relationship of fatigue and practice in a single test. In general, this problem reduces itself to the determination of the place for the 'most favorable pause.' This pause is determined with respect to length and position in the work period by a calculation involving three principal factors—practice, fatigue, and Anregung. The formula that expresses the 'most favorable pause' when all the factors are considered is the ratio between the estimated amount of work done just preceding the pause and that which is done (estimated) just following the pause; the estimations rather than the actual figures being the criterion, because of the opposing effects of the various factors involved.

The actual place of this pause in any series of tests varies with the individual even in tests where the different subjects are working on the same sort of work. The individual characteristics that have most weight in determining the place of this most favorable pause are the capablitiy of practice, power to retain practice effects, and susceptibility to fatigue, with some indications that the grade of practice attained also acts on its location. Wimms (107) finds that 'improvability' and 'retentivity' are positively and negatively related in about an equal number of cases. The length of this most favorable pause when determined gives us knowledge of the power of recovery in the individual. Its place with reference to the beginning of the experiment indicates the control power of the subject by a balanced relation of the factors mentioned above (Gewöhnung). And its distance from the pause that is preceded by an amount of work equal to the amount which can be done immediately following, the Gleichgewichtspause (this pause precedes the günstigsten Pause), determines endurance.

We may illustrate some of these relations by the following figures, taken from the work of Amberg (1) and of Rivers (76). Amberg found that a five-minute rest following thirty minutes of work was slightly favorable. A fifteen-minute rest following the same period of work had no effect. On the other hand, a fifteen-minute rest following sixty minutes of work was favorable in its effects. Alternating five minutes work with a five-minute rest period was unfavorable in the early stages of a two-hour test, but became a favorable pause toward the close. Rivers found that thirty minutes' rest after the first thirty minutes work resulted in a complete recovery. The same rest period following the second thirty minutes of work failed to produce a complete recovery, but sixty minutes' rest at this point was entirely satisfactory. These results are purely individual, and so far the investigations have brought out no specific place for the location of a favorable pause of a definite length. In a later article by Kraepelin (50), it is suggested that the progress of the first fifteen minutes of a piece of work as homogeneous as the addition test, may possibly be taken as a partial indication of the susceptibility to fatigue, i. e., when related to the length of the 'most favorable pause' at this point (this last to be determined experimentally). In Lindley's (57) subjects the length of the most favorable pause varied from fifteen minutes to sixty, showing the necessity in Kraepelin's judgment for the determination of this pause for each indivdual. In other words, it is essentially an indication of personal peculiarity.

Types of curve.—With respect to the work curve as a whole, Kraepelin finds five somewhat variable types. Two are known at the positive and negative practice curves, where (1) the practice effects overbalance any opposing influences to the very close of the two-hour test, or (2) fatigue is the more prominent influence from the beginning. The first curve is based on the work of Ebbinghaus and those tests where the practice attained before the test is very slight or the material is of considerable difficulty. The third 'fundamental form' of the curve is indicative of slight practice, and shows 'spurts' of speed, or practice effects, and fatigue effects within the progress of the curve, but corresponds in general to the first type. The other type indicates the balanced effects of fatigue and practice. To simplify the statement we omit here a discussion of other influencing factors. The fifth is an intermediate form based on the factors in one, three, and four.

Only one of the smaller variations in the height of the curve is considered regular enough to be related to the conscious process. That is related to 'variations in attention' and is therefore a wave of a length of about two and three-fifths seconds from crest to crest. This wave is the one described by Voss. Among the other variations given special notice, is a series that precedes the highest point in the curve; or the preliminary fall due to fatigue that precedes the final effort to keep a maximum of production. The final and beginning Antriebe are noticed. Other variations are grouped under the term 'Störungsantriebe.'

A different problem attempted by these investigators is that relating to the effect of one kind of work upon another immediately following it. Bettman found that the effects of adding and of walking were practically identical upon the intellectual work that followed. Choice reactions were slowed by the first and augmented by the second, but if the 'muscular'[1] reactions are counted and evaluated, both sorts of work actually raise the mean variation and decrease the speed of the reaction. Verbal reactions, reading aloud, adding and memorizing digits, all tend to slow the additions that follow. The heightened muscular irritability after the walk being the only distinctive difference between purely physical and mental work in their effects on the following work. The general conclusion is that physical work raises the motor excitability and produces a large increase in the errors made, while mental work tends to reduce motor irritability. The average times when the directions are actually observed, for example in a choice reaction, are reduced in both cases through the influences of fatigue. 'Premature reactions' produce the apparent difference in time of the reactions that follow the physical work.

Introspection has had practically no place in this type of experimentation, and little beyond a few cursory remarks in any of the material reviewed. One reason is of course apparent, the method of experimentation has not allowed pauses for the report of particular conscious changes, and those reports that are gathered at the end of a two-hour test, for example, are of little or no importance for the early or intervening stages of the experiment.

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[1] By 'muscular' reactions, Bettman (8) means those reactions that are premature, or are simultaneous with the stimulus and are thus not the result of an actual choice.

Wimms (107) attempted to gather some introspections from the school children he tested and reports that the method and the results seem of considerable value if the introspections be constantly checked and are made upon a large number of students. He obtains reports of a gradual increase in the 'feelings of fatigue' and in the difficulty of the task. These general statements are accompanied by reports of such specific facts, as 'muddled' conditions, a desire to sleep, weariness and muscle fatigue, all points of great interest if we are ever to correlate the conscious facts with the objective results or are ever to get any conscious facts regarding continuous mental activity. Similar reports are mentioned in Sikorski's paper. The muscle fatigue was in this case speech-motor. Galton's questionnaire shows the wide range of material possible in this connection. Joteyko (47) believes that intellectual fatigue arises suddenly in contradistinction to the slow rise of muscular fatigue; a belief that doesn't seem to be supported by the major portion of the work that has any bearing on the point. To cite a single instance, it seems from the experiments that there is no certainty that voluntary muscle fatigue, or what passes for muscle fatigue in the normal organism, is any more gradual in its rise than is the appearance of a 'sensation.'

General conclusions.—This incomplete and very imperfect survey of the work of Kraepelin and his pupils indicates the great importance that must attach to the objective record in the estimation of qualitative and quantitative changes in mental work. The distinct failure of the objective record as it has so far been applied seems to be its inability to cope with the situation more in detail. It fails to account for the variations as they seem to appear in the most cursory observation of different persons in their habitual methods of working. Comparability of conditions is absolutely necessary for the comparison of records from different individuals. It is also necessary to determine whether the imposition of, for example, the highest rate of speed, a certain kind of subject-matter requiring a single type of imagery, or a specific mode of reacting, or even of recording, does not alter the conditions more materially than they are otherwise equalized by the setting of certain objectively similar conditions. One person may be affected by the set of conditions imposed so that his imagery is of a type not ordinarily used, another finds that the demand for a high rate of speed is inimical to his usual practice curve, etc. In short, an investigation of the individuals of a group must be made on the basis of their habitual methods of working before work curves can be made of definite value in any series of generalizations.

The question of habituation is one that relates itself to fatigue in an intimate way, though we can merely touch its problems at this point. From the side of physiology, the investigator has begun to present us with all manner of rhythms by which organic processes are regulated, and among the latest of these are the larger muscular rhythms. The facts regarding the number of nerve impulses that a muscle will respond to separately within a given time have been known for a number of years. The relation of the load to the working power of a muscle has also been a matter of common investigation. But the majority of the investigators on fatigue in both fields, muscular and mental, have worked with the express purpose of obtaining fatigue in the shortest time possible. Thus any larger rhythms that might have been present under normal conditions have been obscured by this specific aim of fatigue experimentation. It is a matter of common observation that the workman has his individual speed. The work of Marsh (60) lately has shown distinct daily rhythms. The possibility of shorter rhythms lying between these larger periods and the short attention waves has only lately begun to receive special notice. For example, the work of Hall has shown that there is a definite load and rate for the muscle that will enable any single set of muscles to perform their work for practically indefinite periods of time; so that under these conditions, which he speaks of as normal, muscle fatigue of the exhaustion type is eliminated. Habituation is, in fact, the establishment of just such 'physiological' rhythms, and work done under conditions that do not approximate these habitual rhythms or attitudes is abnormal and does not present to us a clear picture of the properly working organism.

Psychological rhythms follow with considerable accuracy the general picture we have drawn for the unconscious ones. Fatigue, exhaustion, weariness, total distraction, break up or destroy such physiological rhythms. Considerable work has been done to show the effect of varying amounts of distraction upon any piece of work in its various features. Distraction of a very limited amount tends to aid in the production of work; music aids in the maintenance of a rhythm; greater distractions, or those that are incongruous, are fatal to the rhythm (17). The introduction of these slight distractions tends to remove the 'feelings of fatigue' where they are of an inconsequential nature, and in cases of high mental excitement, not merely in inurement as Bolton (12) maintains, these warning signals may be disregarded altogether, disregarded in the sense that they are not present consciously till exhaustion occurs.

In connection with this problem of activity, we have so far failed to mention, except in passing, the 'feelings of effort' and the consciousness of strain that accompany any sort of work that is slightly different from the normal or varies from the purely habitual. These feelings are the accompaniments of both muscular and mental expenditure of energy and are usually conceded to be peripheral in their origin, arising in normal conditions almost entirely from stimulation within the proprioceptive group of receptors. Waller (101) makes the 'sensations of fatigue' simply extreme forms of these sensations.

Mosso showed with the ponometer, a modified ergograph, that the amount of effort expended in raising a weight increased rapidly during a muscle fatigue test. Trêves (95) working on the same problem found the same general result, but finds in addition that the expenditure of effort is in no degree so regular as is the inverse change in muscle work. Effort is expended in waves of force that approximate what he calls the irregular progress of mental fatigue rather than the regular decline of muscle power. He finds that a muscle under strain, such as continuously supporting a weight, withstands the strain steadily until exhausted. This applies to an extreme strain rather than to the moderate forms of muscle tension. These results correspond fairly well with what has been done, principally by way of observation, in the field of mental effort. Exhaustion is always a sudden phenomenon and the more moderate rates of energy expenditure are, so far as concerns the objective records, irregular in their character.

III. EXPERIMENTAL. PROBLEMS AND INVESTIGATION.

A. Some Specific Problems.

As one works over the material in each of the particular fields, the special need seems to be for more and more experimental work. The moment we get beneath the surface complexity, there appears a multitude of individual problems, each offering, apparently, its quota of material for the solution of the more general problems. The need is now, however, not only for more work but for a specialization in the various fields. The data, as will appear from the above sketch, that must be presented within a review on 'fatigue' are entirely too complex at present to admit of unification. So many divergent paths are presented that the thorough examination of any one has so far failed to attract any considerable number of investigators, and until a division of labor occurs there seems little chance of the consummation sought by Seashore in his plea for a better technique in this line of work.

Intellectual and physical work.—In differentiating between intellectual work and physical work there are said to appear certain very distinct characteristics. In physical work, the characteristic usually assigned is the gradual decrease in the curve of work. This gradual decline is assumed to indicate the way in which a muscle becomes exhausted. When the energy of contraction is taken into account, the curve of work becomes more irregular and shows variations that are assumed to be indications of the central changes. The muscle therefore is supposed to fatigue at a steady rate and the motor responses of intellectual work are also expected to fatigue in this straight line curve.

When we come to intellectual work there is reported a distinct difference. Joteyko says that intellectual work goes on without any apparent diminution in ease, or in loss of practice, or in increase in errors. She finds however, that 'sensations of fatigue' appear suddenly. The sudden appearance of these sensations is what she calls intellectual fatigue.[1] Trêves indicates suddenness as the prime difference between nervous fatigue and muscular fatigue. The principal difference then that appears between these two types of work is one of slow decline in the case of physical fatigue and of sudden appearance of certain distinct sensations in the case of nervous fatigue, fluctuations in the 'objective manifestations of work.'

Kraepelin finds that the loss of capability, while it appears at a certain stage in the work, is nevertheless due not to a sudden onslaught, but rather to the gradual decline in capability to retain practice effects and to maintain the work at the same high rate attained at earlier points in the two hour work curve. He finds that practice increases in a decreasing ratio and that fatigue increases in an increasing ratio. The result of these opposing influences is that the curve at a certain point changes from a rising curve to a falling curve. This fact does not indicate the sudden appearance of fatigue, but indicates the point where fatigue overcomes the practice effect. Sensations that are to the subject 'fatigue sensations' are simply signs of weariness and due to the monotony of the work. They can be pushed aside and the work continued for some time at higher speed. Intellectual or central fatigue comes when the curve of work begins to fall and cannot be brought back by any effort of the subject.

The difference in these two views seems to be plain. The difficulty is, in the first instance, one of statement merely.

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[1] A question arises here; does Joteyko mean in the case of intellectual fatigue, merely 'sensations of fatigue,' or the actual loss of capability to work? If she means sensations alone, then certainly she is right; for all sensations, as far as the psychologist can tell us at the present time, arise suddenly; they are due to the shock change which occurs in consequence of the presentation of stimuli. But if she means a loss of capability to do mental work in terms of nervous energy fluctuations, then we would question the accuracy of her observation, in so far as it applies to the objective manifestations of work, at least.

Kraepelin is basing his conclusion on a single series of addition experiments and on the definition of fatigue which says that fatigue is a loss in the ability to do intellectual work. Joteyko's definition of fatigue refers seemingly to the sensations alone, and not to the ability to do work. If we try to combine these two definitions, we still find, however, that there is a distinct difference of opinion.

The work curve as Kraepelin investigates it, is found by a combination of the methods employed (*a*) in a purely intellectual effort without recording and (*b*) in making the motor record of intellectual effort or of physical effort. There ought then to be, as far as results are concerned, a distinct difference in the type of curve. The intellectual work curve ought to show more variability. It ought to show the entrance of disturbing factors such as change in attention, and the rise of different ideas; and it ought to show the effects of sensory or motor fatigue.

The further question arises. Does sensory fatigue, so far as isolable, show any different characteristics from mere motor fatigue? Few curves of purely sensory fatigue have been worked out as yet. Where sensory fatigue has been determined, however, the curve is more or less a straight line curve. This appears in such curves as are obtained from the olfactory senses, from taste, and similar sense fields. In the reaction time experiment we have a case where both sensory fatigue and motor fatigue are possible. Scripture shows that sensory fatigue is a prominent factor. Eliminating the motor elements one by one, he shows, as has been mentioned above, that the curve of fatigue as shown by the increase in length of reaction time and by increased variability of the time of reaction, or mean variation, is decreased very noticeably. This curve is practically equivalent to the muscle curve.

Objectively we can undoubtedly arrange mental calculations and associations so that they will give, when recorded, practice and fatigue effects that make a straight line curve as Kraepelin has done, i. e., taking them separately. But there might go along with this the greatest possible number of fluctuations from the side of introspection. Or the reverse is possible, to set such a task that introspectively the subject *feels* that he is maintaining a steady standard in rate and quality of work and with this obtain very considerable changes in quality and quantity of work objectively considered. To repeat, sensations of any kind are more or less sudden in their nature; they do not come gradually into the field of attention. If these alone be fatigue, then our problem is already solved. If, however, we are dealing with a real diminution of work, our problem remains. *Does the appearance of 'sensations of fatigue' bear any relation to variations in the objective work curve?*

Relation of the 'muscle fatigue' curve to the 'intellectual fatigue' curve.—The conditions so far discussed are conditions which are maintained in both intellectual and physical work. The intellectual work is not itself recorded as actually done, however, but only the motor reactions which are made in objectifying the records. The result of this situation has been entirely to obscure the greater portion of what may be genuine intellectual fatigue phenomena. Lombard, Trêves, and others have shown the complexity that lay hidden in the 'straight line' muscle curves of voluntary contractions. In some few instances, such as the reaction time experiment, we can see that intellectual fatigue shows itself not merely in sensations of fatigue, but also in increase in mean variation, in an increase in errors made, and a possible decrease in the amount of work done. This quantitative decrease does not show itself as definitely as do the other phenomena. The reason for this seems plain. In cases where a motor record of the experiment is made, the motor portion of the whole sensory-motor arc is the part which controls. The intellectual 'give and take' of the situation is obscured by this necessary and fundamental response factor.

Let us see if a situation similar to this does not also obtain in much of the work so far done in muscular fatigue. In Lombard's investigations the curve starts and works down in a 'straight line' curve to the point of no contraction; then starts over a second time and so on, making almost any number of these 'straight line' fatigue curves. This rise and fall occurs in a single experiment. Mosso claimed in his early experiments that to get the same fatigue curve over it was necessary to wait two hours or more after the first experiment. Naturally he did not mean that the muscle could not work during this period. But he did mean that the curve was materially changed until after two hours' complete rest. Lombard found that the curve could be run for twelve or fifteen minutes and still obtain contractions one-half or two-thirds as large as in the beginning. The phenomenon of the 'Treppe' still further modifies our idea of the 'straight line' muscle curve when the muscle is working under approximately normal conditions.

This situation seems to indicate that we must materially modify our idea of the fatigue curve in muscular work. W. S. Hall (39), in a series of experiments mentioned above, finds that if the conditions of ergographic experimentation are modified slightly, the exhaustion 'fatigue' curve does not appear at all. What the situation would have been had Mosso used such a type of experimentation in testing the effects of mental fatigue on muscular work we do not know. We do know in Lombard's experiments that voluntary effort ceases for only one or two contractions, and it begins again with a vigor which is almost equal to the primary effort.

In Kraepelin's experiments we find that the method of recording is distinctly different. Kraepelin himself makes no remarks with regard to his method. We learn, however, from the descriptions of his experiments that what actually takes place is a very rapid movement of the fingers and of the hand in recording the additions made by the subjects. In the tapping experiments carried on by Dresslar and others, we find that the fatigue curve is quite similar to the actual 'muscle fatigue' curve as made by the ergograph under modified conditions. Is not the fatigue curve that Kraepelin obtains, a curve that is made up in great part of purely 'muscle fatigue' phenomena? Amberg thinks not, at least in those subjects where the recording is relatively faster than the calculations, but he only finds one such case. However, the amount of work necessary to record one figure every time one is added is not inconsiderable. And in addition to this it takes up no inconsiderable part of the time of the entire experiment. The work recorded below in the experimental portion of this paper shows quite plainly that no simple motor response can act with a speed equal to the details of mental imagery. This last factor is not definitely met in any of the discussions of these experiments.

The other factor is, however, the one we wish to emphasize here. Where only one or two fingers are used and a small portion of the arm, does this motor reaction not take on practically the exact conditions of the muscle exhaustion 'fatigue' experiments? And if so, may we not expect to find a gradual decrease in the actual amount of work done due, not to any 'intellectual fatigue' induced, but rather to an actual muscle fatigue? If so, we may expect to find that the curve is a regular one and takes on the characteristics of the simple *muscle exhaustion curve.*

The only modification of importance that would appear in this curve is one due to the large practice effects that always appear in this kind of work. In the ordinary muscle curve, inurement effects do not appear except when the experiment is carried over a long period of time. That they do appear, however, when experiments are made long enough, is shown by the work of Bolton. The practice effect in the case of additions is in the first experiment a very large one. None of the subjects in Kraepelin's tests carry the work far enough to reach what is known as the automatic stage in the additions. The results show that practice effects were prominent in each day's experiments. The Uebungsspur also appears in the effects that remain from one day to the next up to the very close of the series of experiments.

The difficulty in evaluating Kraepelin's experiment rests, in addition to this fact, upon the lack of data regarding the number of errors made by the subjects used. The *quality* of the work was not taken into account in any of the tests. Exceptions to this extreme statement may be noted in the work of Amberg and Voss, where both make specific mention of errors, Amberg to the effect that in his work the errors are only a slight per cent. of the total additions, giving as the highest per cent 0.0917 per cent, and for corrections, 5.38 per cent of the whole. Says Voss in this respect:

> Wir glauben das thun zu durfen (omit errors) da einerseits die Deutung der gefundenen Fehler, nicht in das Gebiet unserer Arbeit gehört und da anderseits wie Amberg best gestellt hat, die Fehlerzahl schon an und für sich gering ist und mit der Uebung noch stark abwinnet.

I do not find that Amberg is at all certain he has shown this last point, for he finds the errors too few and scattering for any definite conclusions.

Apparently, the motor reactions of any subject, if made complex enough and not held to an exhaustion series of reactions, will give a curve similar to the fatigue curve that Kraepelin finds. Practice effects may continue up to the close of the experiment, and no fatigue effects appear in the curve. For example, when eye muscles are eliminated one by one, variations due to fatigue decrease in the reaction test. The practice effects, however, remain, and the reactions become shorter and shorter up to the time when the experiment closes. *The first general problem before us then is to determine what are the true conditions of a purely intellectual fatigue test, if such there be.*[1] Naturally these conditions cannot be determined except through a long series of experiments. So in the work here reported the conditions selected were such that we might decide to some extent at the close of the experiment whether we had obtained any new set of conditions by the change in the results obtained. The problem as thus stated is not one that we hoped to solve outright at this point, but rather to attack with the view of obtaining a method. In this respect, the paper follows in a greater or less degree the suggestions made by Hall and by Mrs. C. R. Squire (90) in their work on fatigue tests, as well as the work of Kraepelin, Sikorski, *et al.*, who have attempted to separate the conditions necessary for practice improvement from those appearing in fatigue and inability to retain practice effects.

Quality of work done.—In any case there is a necessity for making a record of the intellectual work done, if either the quantity or quality of that work is to be measured. If we are simply seeking for quantity, it is possible to get a fatigue curve by the ordinary methods of recording and of carrying out the experiment. By this we mean that the motor reaction may be used at the highest speed possible. *When used at this maximum speed, our contention is that as far as quantity is concerned the predominating factor is a curve of simple muscle work only slightly modified by the sort of intellectual work recorded, and the muscle curve so obtained is not the significant one as far as the intellectual work is concerned.* For a fatigue curve which is an indication of the quantity and quality of the intellectual work itself, we must reduce this purely motor response effort to a minimum. Further, this reduction of the motor element cannot be achieved, if the record be made by any of the more mechanical means, or if it be made at the highest speed of which the subject is capable. It may be possible to use the usual methods of recording if the speed at which the work is done is made what may be called, following Hall's phraseology, a 'physiological' speed. The only method we know of getting this habitual physiological rate in the case of making a record of intellectual work is to allow the subject to choose his own speed.

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[1] Bonser's (13) investigations indicate the value of such researches though the actual results so far obtained are necessarily small.

Fatigue and practice.—A second problem deals with the relation supposed to exist between fatigue and practice. Kraepelin's conclusion seems to be that these two factors are definitely correlated. Our first question may be stated thus: *Is fatigue actually correlated with practice in the way in which such experiments seem to indicate?* This question applies to fatigue of the type known as loss in power to produce work, a decrease in capability. The second question relates to the matter of errors as they appear in work where errors can be evaluated, and may be stated as follows: *Do errors as they appear in the work, come in an increasing number per unit of time, or do they decrease as practice effects begin to appear;* in other words, *are the errors an indication of the results of practice, rather than an indication of the stage of fatigue which the subject had reached in any one experiment?*

It may be well to state in this connection that certain other results obtained and mentioned above, such as Thorndike's and Wimm's, do not wholly agree with the results indicated by Kraepelin. The three hour test used by Thorndike shows no fatigue effects at all. In the experiments on school children, he finds that the errors increase as the work goes on, but he also finds that the amount of work done per minute increases. Going back to the work of Ebbinghaus and those experimenters who have attempted to use purely intellectual tests with no, or very little, motor recording, we find that in the majority of cases the number of errors increases as the experiment is continued. In about an equal number of cases the experimenters find that the amount of work done at the end of the period of the experiment is equal to, or greater than, the amount done in the beginning of the experiment. The other results seem to show that the amount of work decreases as well as the mean variation in number of errors.

Evaluation of errors.—Another problem closely allied to these relates to the appearance of errors in any single memory experiment. Do they constitute any distinct relation to the practice curve or to fatigue as a loss in capability to do work, or are they independent of both? In either case, it certainly seems an important problem to determine what the facts are and whether any test for them be obtainable. As a secondary problem, we may inquire whether the errors as they arise change in their character from the beginning of the experiment to its close. Do they increase in number; are they more prominent and numerous at the beginning of the experiments or at the close; is their distribution due to the effects of practice; or are they scattered throughout the series without any reference to the work curve as far as amount of work done is concerned? The major portion of the errors will doubtless depend for their quality and number upon both the actual difficulty of the intellectual work itself and certain changes that occur in the effects of practice and fatigue throughout the series. The correlation of quality of work with practice and fatigue will necessitate an attempt, at least, to classify the kind of errors that occur.

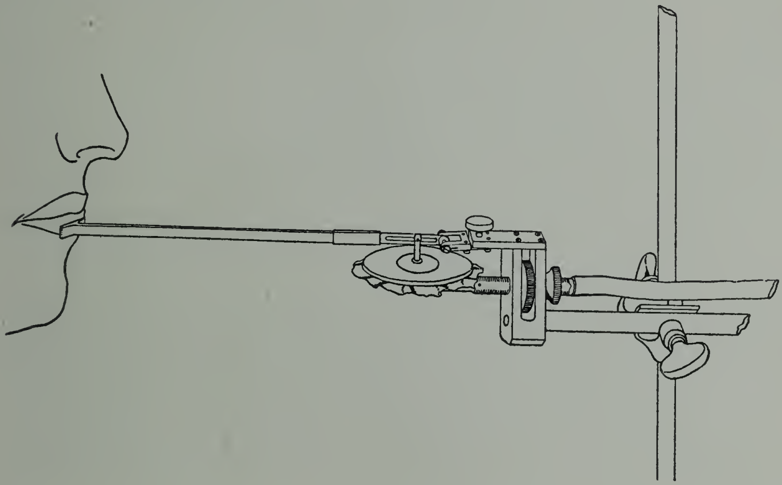
To summarize: We have sought to obtain a method of recording that will eliminate almost entirely fatigue of the muscles involved. This is further to be provided against by using habitual rates of correlation in thinking and recording. The mental material or work is simple, but difficult enough to give distinct qualitative results. As a result of these conditions, we hope to get definite answers to certain questions. Is there a difference between mental work at laboratory speed and that according to established habits? What further can be said about the actual nature of the work done, i. e., with respect to quantity and quality? Is there any value in persistently demanding introspective accounts with such experiments?

B. *Conditions Selected for Experimental Tests.*

The specific arrangement of the experiment is quite similar to that used by Mrs. Squire in her tests on fatigue in this laboratory. We used practically the same mode of procedure with certain changes and modifications noted below. The test is primarily a memory test.

A series of ten digits was memorized and tapped; a typical one being as follows: 4, 4, 2, 1, 3, 2, 2, 4, 3, 1. By 'tapping a digit,' or a series, is meant the movements of lip or finger made in groups or series of groups and finally recorded by means of lever and tambours on the smoked paper. In the series above, for example, four movements of lip or finger were made, and after a slight pause four more movements followed. Again a slight pause and the two movements corresponding to the third digit of the series were tapped. This procedure gave on the drum record a group of breaks in the pointer line that corresponded in number and order to the number and order in the series memorized before the experiment began. The movements of the lips themselves in the lip tapping correspond almost exactly to the *first part* of the movement involved in closing the lips for the letter 'b.'

Apparatus and Method.—There is little need of a detailed description of the apparatus used in making the record. The major portion of it is that which is wholly familiar in every laboratory. The Marey tambour with a wooden lever about five inches long, transmitted lip or finger movements to the smoked paper. The connection with the lip, lower lip usually, was made by a narrow strip of paper or metal so attached to the end of the lever that it could be changed for each subject. This strip was about one centimeter in length and one-third as broad, and was fastened at right angles to the tip of the lever of the receiving tambour. It was kept in position on the lip of the subject by the amount of pressure needed to make the subject conscious of this movement in connection with the number of taps to be made, four, six, etc. After the preliminary trials the subjects report no feeling of discomfort or of fear lest this should become disarranged, and it could even be shifted during a series without confusing the record.



The receiving tambour was held in position by an ordinary standard and universal clamp and could be arranged by the subject to suit his own particular habits of sitting while reading or writing. It could also be shifted during the experiment if the subject so desired. This last possibility was needed but a few times, as the subjects found the position could be arranged quite satisfactorily in the beginning of the experiment.

From this receiving tambour, rubber and glass tubing enabled us to transfer the record to any distance desired. After some preliminary experimentation in which different tables were used and even different rooms, it was found that the apparatus could be made to run regularly enough to allow all of it to be placed on a single long table. This final disposition of the apparatus was an arrangement that put the recording drum but a few feet away from the subject, either on the same table or on another standing near. Sight of the drum or the record was prevented by a large paper screen placed about two feet from the subject. The table on which the apparatus was placed was a large heavy one of oak, and very solid and steady under any jars of the apparatus or of the motor. Practically nothing of the vibration of the motor was transmitted to the subject.

The drum used in recording was the Scripture horizontal drum with wheel attachment for leather belting. To drive this drum at as uniform a rate as possible, a small motor run by electric drop-light attachment was used. The Pillsbury speed reducer enabled us to control the rate of the drum and conserve the use of smoked paper. This was further cared for and the records made more uniform, by running an endless piece of the smoked paper over the Scripture drum and a second drum at a distance of about seven feet from the first on the same table. This made a sheet of about fifteen feet of smoked paper, and enabled one to sit down at the apparatus and run off a record thirty minutes in length without any change in adjustments. The paper was smoked in the ordinary manner after being placed on the drums. After the record was made it was floated with the shellac and alcohol preparation and allowed to dry before being taken down. With paper of the right thickness this can be easily done by applying the preparation to the reverse side of the record, as is done in charcoal work.

Along with the record of the lip or finger movement, a time record was made. This was done by putting into single circuit a Jaquet time-marker and a pendulum clock with electric attachments. The first marked seconds, while the second was set to mark the half minutes. They were adjusted to run together so that the record as it appeared on the paper gave the usual short break each second, and at the half minute gave one somewhat longer. This method of recording enabled us to detect variations in the record within the time of a second. This time record obviated the necessity of taking account of any possible irregularities in the rate of the motor.

The clock was kept in another part of the room, and all the apparatus on the table was muffled by cotton batting and cork supports. This was not entirely satisfactory since in one or two instances the subject reported at the end of the test that he consciously selected the rate of the throbbing of the motor for his own tapping rate. This point was examined in the records of other subjects, and kept in mind in reading the rate of tapping. No selection of the rhythm of the motor could be detected either in a regularity among the records of the subjects, or in any sudden and continuous change in speed in any single record. In the majority of the records the irregularities are of such a nature as to permit of no such possibility. Even if such an objective rate were selected, the results obtained would only be made more definite and certain in their intrinsic character. Those records run with the motor in another room showed no differences in this respect. In but one or two of the series did a subject report consciousness of the movement in the apparatus or of the noise in the room. This last was reduced entirely to that arising from the apparatus, since the experiments were all carried out in a large basement room free from noise beyond that incidental to any room that is not entirely sound proof.

Material of Tests.—The experiments may be divided into four different groups. The *first* (I) was a practice series intended to acquaint the subject with the apparatus, to test the effects of practice on work carried on at the will of the subject as far as speed was concerned, and to test the effects of practice on the changes in number and kind of errors. This series in other words acted as a control test.

The *second* group (II) consisted of seven experiments, of somewhat irregular character, arranged to determine the effects of practice and the increased difficulty of any problem set, on the results of the work itself and on the kind of fatigue produced. 'A' of this group was the series of ten digits used in the preliminary practice series and constituted a second control in a portion of the subjects used: in the others it merely marked the first of the tests after a short series had been used as a practice series. These will all be indicated when the results are tabulated. The other six of the group, 'B,' 'C,' etc., with the exception of the sixth series of the group, are much more difficult than this first one. They consist of a series of consecutive digits, ten in number, that are to be memorized before the tapping is begun, and *also* two or three calculations that are to be performed on the series during the experiment. The kind of calculation to be used was communicated before the test began, and memorized, so that no directions were needed by the subject during the course of a single experiment. This last condition is entirely true of the 'B' and 'C' of the group only. In the remainder of the seven the conditions are somewhat different. *Two sets* of ten digits each and the calculations that went with one of them were memorized by the subjects. No trials were made where both series involved calculations. The subject was started on one of these series and allowed to continue till the word 'change' was spoken by the person in charge of the experiment. At this point, the subject was expected to begin the second series which had been held in memory till then.

In detail the series used were as follows: as practice series and the first or 'A' of the regular tests, the series 4, 4, 2, 1, 3, 2, 2, 4, 3, 1, was used; it also served as the first part that preceded the word 'change' in series 'C' and for that part of the series that followed the direction to change in series 'E' and 'D.' For series 'B,' the following served:

3, 2, 2, 4, 3, 2, 4, 4, 2, 3,
subtract one,
multiply by two and subtract one,
multiply by three and subtract three.

Series 'B–2' was as follows:

1, 1, 4, 2, 1, 3, 2, 2, 1, 3,
add one,
multiply by two and add one,
multiply by two and subtract one.

Series 'C' began as described above, and at the signal changed to the following:

2, 1, 3, 4, 1, 1, 2, 4, 2, 3,
multiply by three and subtract one,
add three.

Series 'D' ended as described above, and began with the following:

3, 1, 2, 1, 4, 3, 4, 1, 2, 1,
multiply by three,
multiply by three and add two,
multiply by two and add three.

Series 'E' was much less complex, consisting of twice the original series and at signal the original. While series 'F' was the last of this group and probably as far as complexity was concerned the severest test of the group; the first part of the test was as follows:

3, 1, 2, 1, 4, 3, 4, 1, 2, 1,
multiply by three,
multiply by two and add three,
multiply by three and subtract two.

This was continued till the signal to change was given, when the following series was taken up and tapped, having been committed to memory before beginning the tapping of the first part of the test: 2, 4, 2, 1, 2, 4, 1, 3, 3, 2, with no calculations.

In this group, group II of the tests, 'A' was intended to serve more or less as a control, as it was used by some of the subjects for the first time, and by the others after the practice group. 'B' and 'B–2' constituted two of the same character and if possible were to be used in evaluating the effects of practice in any single test. In the others an effort was made to eliminate practice and to test the effects of a transition from a difficult piece of work to one that was more or less habitual (in some of the subjects practically automatic), or the reverse transition.

Eight subjects were used on this second group of tests, five of whom had practiced on the first group mentioned above. They range in types of imagery from the highly visual to the highly motor; the range being practically the extremes of these two types. One of the subjects is so definitely motor in his type that he is unable to find any other sort of imagery in his thinking. None of the subjects used were highly auditory, though in some cases this kind of control was used in remembering the series. All of the subjects were graduate students and instructors in the department. All were therefore trained introspectionists and capable of carrying on an experiment from the standpoint of the interest of the work, rather than needing an intrinsic interest in the character of the work itself.

The third series (III) of experiments here reported was of the same type as the above, with this exception in the conditions of the test itself. The subject was directed to memorize as above and carry out the calculations indicated, but instead of selecting the rate that seemed most favorable to the continuance of the experiment, he was asked to carry on the tapping as rapidly as possible, compatibly with avoidance of tetanus of the lip or finger. This is the usual direction in tests of this kind and was used here as a control over any possible irregularities (or regularities) that might arise through imperfections in the apparatus and thus cause an artificial record; secondly, it was used as a control test for the former series.

A final series (IV) was attempted in which the subject did the same sort of work as in group two, under all the conditions there described, with the exception of the manner of making the record. Instead of using simply the lip or one finger in making the record, the subject was asked to tap the series with both the hands and the lips in turns. The forefingers of each hand were used and the lower lip as above. These records were not made simultaneously but alternately, and went into the same line of the tapping curve, thus giving, not three records, but one as before. It was possible to determine which movement had made any portion of the record by arranging different degrees of tension in the tambours and by an arrangement of the experiment which asked for the alternation of hand, lip, hand in a definite order. Only three subjects carried out this test in detail.

The time asked of the subjects in these experiments was rather exacting in all the groups, and in group II especially where the subjects were asked to come to the experiment immediately after at least one hour of exactly the same sort of work (the laboratory, library work, etc.) and at the same hour of the day for the seven consecutive days used in the series. The remainder of the tests were not carried out so rigorously, but the same time of day was insisted upon for the completion of any particular series or group. All of the subjects did not come at the same hour of the day but hours were selected, generally in the forenoon, when their work was regular enough to admit of the above directions being carried out, or in the afternoon, if the subject regarded himself an afternoon worker. No outside artificial work was imposed upon the subjects at any time, though the subjects were asked as far as possible to make the days on which the groups came as nearly typical as possible, and where this was not carried out through unavoidable circumstances, special note was made of the condition in which the observer came to the experiment. No specific diet was suggested, but those things that the subject considered especially stimulating or 'fatiguing' in his own case were omitted from the bill of fare during the continuance of the group of experiments, or when not so omitted noted in the preliminary notes.

Of the persons who gave of their time and energy to this work, A., Sd., Rhn., and Ym., were men. Ch., Dy., Rn., Fl., and Wl., were women.

This type of test handled as it was is primarily a memory test. The introduction of the calculations into a number of the later series modified this slightly but mainly prevented complete automatism. The subject was expected to memorize the series of digits before beginning the tapping. When the calculations were introduced he memorized, in addition, the *processes* that were to be carried out during the experiment. He did not at any time *perform* the calculations before beginning the tapping of the first series of digits.

Regarding the length of a single test there is some question. Undoubtedly a few of the tests ought, for purposes of control, to be run to the length at which we should expect to find distinct results of mechanical fatigue or of exhaustion. None of the problems as we have stated them above actually demand such results. It was decided, therefore, in carrying out the majority of the experiments not to run them beyond the time selected by Kraepelin as sufficient to indicate the general nature of fatigue in the individual subject. This time as he determines it is between fifteen and eighteen minutes after the beginning of the experiment. To be sure, in so arranging the experiment we do not get the actual work curve. What we do get, however, is a chance to determine the *tendency* in all of the characteristics mentioned above.

The length of the experiments was varied for the subject each time. There was thus no chance of a final spurt or attempt to eliminate errors as the subject felt the end of the test approaching. The actual time spent in making a single record varied from fifteen minutes to thirty minutes. The subject at no time knew exactly when the experiment would be terminated.

An attempt was also made to eliminate mechanical practice by giving a series of practice tests on a simple series of digits for eight to ten days preceding the actual experiments which were to be used. This practice series will be referred to in the report made on the tests. An attempt was made, however, to keep the interest of the subject aroused throughout. All of the subjects, notwithstanding, report wandering attention and actual loss of conscious control over the subject-matter of the experiment. In one or two cases the subjects became entirely confused and were compelled to start again. In some of these cases the cause is easily traceable to the emotional excitement of a new type of recording; for in using the same series of digits with the same subjects and allowing them to tap with the fingers, instead of the lips, they carried the experiment through with no more effort than was reported by other observers. Practice eliminated any conscious lip fatigue in all cases.

Isolated exceptions were plainly due to the arrangement of the apparatus or to the immediate condition of the observer during the experiment. These instances are reported in the introspections that accompany the records.

Collocation of Results.—In determining the factors represented in the results, a large number of tables and curves were made, all of which seemed necessary to determine the nature of the work done and the origin of the changes that occurred. These tables and curves are far too numerous to try the patience of any possible reader with all of them, so from the number we have attempted to select some to show the different methods used in collating the results; and to select others that seemed typical of the whole, or that indicated the relation these experiments bear to the work of other investigators. Finally, certain tables show what seems novel in the use of this method as applied to intellectual work. It is unavoidable in selecting only a few of the large total of tables and curves that some illustrations will be omitted which certain readers might find helpful.

An effort has been made to present these results in the following order. (*a*) The various facts that appear in connection with changes in speed and amount of work done have been discussed first, since these are the most prominent part of the results obtained by other psychologists and therefore must show at once the relation these experiments bear to earlier work. (*b*) In the second place, the facts have been assembled that have come out regarding the "grouped" appearance of errors and their quantitative distribution in the continuous series of work; and thirdly, (*c*) an attempt has been made to determine the character of these errors in relation to their distribution. (*d*) In the fourth place, a partial correlation of the subjective experience of each subject with the more prominent changes in the objective records of the subject is offered in the hope that it will stimulate other introspective accounts. In the conclusions, we have attempted to bring together the general and specific results with reference to some more definite explanation of the phenomena than has as yet been offered.

C. *Detailed Discussion of Experiments.*

a. Practice Tests.

The preliminary trials were carried out in detail on four subjects, five including the writer. These habituation tests were made with the series of ten digits, 4, 4, 2, 1, 3, 2, 2, 4, 3, 1, and were carried out under the conditions set for the experiment and described above. In all of the subjects the tests were continued until the series had been thoroughly learned and any errors that occurred and were noticed, were reported by the subject as due solely to mind wandering. The automaticity of the process finally became so great that it enabled the subject to turn the attention from the immediate process for periods that covered the time spent in tapping two and even more digits. The results of these tests are interesting from the standpoint of practice, changes in rate from one experiment to another, and the variations in errors that appear.

Nature of errors.—The errors that appear in this test are of four kinds; one, where the digit appearing in the record is larger than the proper digit by just one tap; those where the digit or digits are repeated, as 3, 2, 2, 2, for 3, 2, 2, and 4, 3, 4, 3, for 4, 3; a third sort is the omission of one or more digits, as 3, 1, for 4, 3, 1; the fourth, of which a very few occur in these tests, is where one or two taps are made and these followed after a short interval by others, evidently intended to finish the tapping for some digit larger than either of the groups. This occurs for the larger digits somewhat infrequently in these series. Once or twice, but not oftener, less than the requisite number of taps has been made in this test, showing possibly some uncertainty on the part of the operator regarding the proper digit to be tapped. The majority of the other errors come from distinctly different sources. For example, omissions and repetitions are both very uncertainly reported by introspection. The omissions are particularly difficult to catch in this way, while for the writer the over-tapping was practically always detected in these simpler series. Finishing a digit slowly is usually a plain case of conscious effort to orient one's self in the series and in the tapping of the digit.

Little can be said with regard to the rate of tapping at this point in the practice. As the control over the mechanical difficulties of the apparatus and over the ten-digit series increases in accuracy and ease, the rate of tapping gradually increases. All but one of the subjects show that they have taken advantage of the direction to take a rate that is fairly safe and as habitual as the novel character of the experiment permits; i. e., they begin the tapping at a much slower rate than the difficulty of the test demands. One subject, 'R.,' began so near this safe limit in rate that in consequence she does not show the same changes that are exhibited by the others. This subject is distinctly of the motor type in her imagery. No method of questioning that would elicit an appropriate answer and still leave the subjects in the dark with regard to the nature of the experiment succeeded in bringing out any consciousness on their part that there had been any change in rate during an experiment. None were at all sure that they had changed the rate from one day to the next, though all reported greater ease after the first few minutes of a test and after the first days of the group. The 'at will' rate which is a part of the directions for this test shows itself in the loss of the 'Antrieb' and in a relatively much larger 'Anregung.' The other factors of the Kraepelin work curve seem constant.

b. Control Tests in Practice Series.

After this preliminary practice on the simple series of ten digits, 4, 4, 2, 1, 3, 2, 2, 4, 3, 1, the series was complicated by the introduction of a second set of directions. In this particular test the subject was asked to add one to the original pattern, to multiply it by two, multiply by two and subtract one, and finally to multiply by two and add one. When this had been done the whole set of directions was to be reversed twice without repeating the simple series, and at the last reversal of the directions the original pattern was to be tapped and the whole gone over again and again till the signal to stop was given. This made a series of repetitions of a cycle of forty digits, each made up of four inner cycles of ten digits, that had to be repeated four times before the subject was permitted by the directions to return to the original cycle of ten and gain control of the entire series from the beginning once more. Two or three of the subjects found it necessary to return to the original series long before the whole 160 digits had been repeated, in order to get a 'fresh start.'

The records of the three subjects who worked regularly through the whole series of groups will be given here. Two of the others will be mentioned below, in noting the exhaustion phenomena. These three subjects had been over the original pattern on an average of twelve times each and were practically automatic so far as the sequence of movement was concerned. The conditions were kept as usual. The following table shows the individual differences and the changes that appear in the records of this group of tests.

TABLE I, 1.

SUBJECTS	Rn.				Ch.		Sd.	
ORDER OF TEST	a	b	c	d	a	b	a	b
Errors, 1st min.	2	3	1	6	4	0	1	1
2d "	3	2	1	4	0	0	0	1
3d "	4	2	2	2	0	2	0	0
4th "	8	2	2	0	0	0	0	1
5th "	3	4	4	3	0	3	1	0
6th "	3	2	4	4	3	0	1	1
7th "	5	3	3	2	0	2	0	1
8th "	3	4	4	7	0	3	0	1
9th "	7	1	4	2	2	3	1	1
10th "	5	1	1	2	0	1	2	1
11th "	4	1	4	1	3	2	5	0
12th "	1	2	3	4	5	2	0	2
13th "	5	4	1	1	4	4	0	0
14th "	1	1	4	2	2	6	2	0
15th "	4	1	1	4	2	4	0	0
16th "	1	0	3	3	4	4	0	3
17th "	2	3	6	2	2	6	0	0
18th "	1	2	3	1	3	1	0	0
19th "	0	1	1	5	3	5	0	1
20th "	1	6		2	1	5	5	2
21st "	1						2	2
22d "							0	1
23d "							4	0

TABLE I, 2.

SUBJECTS	Rn.				Ch.		Sd.	
ORDER	a	b	c	d	a	b	a	b
1st period	85.2	98	118.4	136.5	78.8	90.6	75.4	92
2d "	78	83.6	119.4	123	81.6	89.4	72.2	93
3d "	81	92.2	112.4	112	86.4	95.6	77.2	92.8
4th "	86.6	95	96	99	97	98	79.5	96
5th "							86.3	106.8

TABLE I, 3.

SUBJECTS	Rn.				Ch.		Sd.	
ORDER	a	b	c	d	a	b	a	b
1st min	107	123	121	150	80	95	74	91
2d "	96	104	116	91 ?	60	93	90	103
3d "	83	90	100	—	88	91	67	94
4th "	78	77	121	133	78	90	75	90
5th "	62	96	134	127	88	84	71	82
6th "						97		

Table I, 1, 2, 3. '1' and '2' are similar to records in practice series; part '3' of the table is to show the changes in rate per minute for the first five minute period of each of the records given. In test 'b' made by subject 'Sd.' the errors for minutes 24, 25 and 26 are 2, 2, 1, respectively. These tables again show merely those points that are characteristic of the whole number of experiments taken. These records were selected to show the range of results and are not so typical as others that might have been selected.

Rates of tapping.—Part 3 of the table gives the rate of tapping per minute for the first five minutes, We find at once the distinct indications of the beginning 'spurt' or 'Antrieb,' which shows itself in the second minute. The second record of 'Sd.' where the 'Antrieb' was somewhat late in attaining its maximum still shows the characteristic decline immediately following. The recovery from the 'Antrieb' begins in some instances as early as the third minute and shows from there on the ordinary influence of 'Anregung.' Inside this general rise there is, however, an important secondary Antrieb. This follows a decline from the first Antrieb and is not due to the definite establishment of the Anregung phase, but rather to a backward swing of the preceding wave. Such a fluctuation may be expected, when the subject is permitted to determine a rate satisfactory to himself. It is therefore of considerable interest in analyzing the formation of the Anregung phase of the work curve. The appearance of a stable condition of Anregung is put off in these tests and in the later ones, till the beginning of the third five-minute period, so that when the rate is averaged by five-minute periods the Antrieb covers the period of the first five minutes, the preliminary Antrieb and the return wave being obscured and bunched as a single Antrieb.

Record four made by 'Rn.' fails to show any recovery that continues throughout the test. Record three made by the same subject shows the recovery but again shows a decline that lasts during the remainder of the test.[1]

Errors.—There appears in the errors something we have not noticed in previous experiments, and had not expected to meet with in these. *The errors per minute show but little if any tendency to drop in number under the influence of practice or to increase through any possible fatigue.* We can only call attention to this point at this place as the tests are not arranged to bring out the reciprocal effects of these two influences. It is impossible to tell whether in this form of experiment, the opposing effects of fatigue and practice,—assuming again that Kraepelin and others are right when they so describe them,—are equal in their influence on the number of errors. Fatigue to be sure has combined with it here the effects of ennui and monotony.

The second point worthy of note is the manifest tendency of the errors to appear in groups. This first came to our attention when the records were transferred to the columns in which the digits were added and the errors marked in red ink. The phenomenon had been noted and the final set of tests made and read when we were gratified to note that the same general phenomenon had been found in connection with some practice tests made on the typewriter by Mr. William Schuyler (81a). The mention is but an incidental one and has not been followed up by those reporting it so far as the writer knows. Counting the errors by groups, every record shows this tendency except possibly the more automatic ones and those in which for other reasons the errors were kept down to a point where they were very few in number. Even in such records, when the character of the rate and other modifying conditions are taken into account, the position of the errors made is easily accounted for on the group principle.[1]

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[1] In record four this subject reported drinking two cups of coffee just prior to the experiment, a stimulant that always arouses her to a high grade of mental excitement. The beginning rate is much higher than was natural and could not be maintained.

In the arrangement of errors as they appear in the earlier records of each subject the grouping is somewhat obscured by what may be called 'accidental errors,' a term that we shall attempt to justify further on. But even here the errors fall into small groups. The most noticeable individual difference appears in the number of errors and their distribution. In subject 'Rn.' the errors tend to decrease slightly toward the close of the test and throughout they show the grouping clearly. In subjects 'Ch.' and 'Sd.' a tendency to increase the total of errors per minute is apparent.

We have given much space to these preliminary tests, since it was here that the facts came out which led to further experimentation. Certain of the results can be shown here as well as in the later experiments. We are thus able to offer the later tests as practically corroborative in respect to these points. Mention was made above of two other subjects who were tried on these preliminary tests, but the records were to be given a separate discussion, on account of certain special facts that appeared. The discussion of those experiments will be taken up at this point, and the further analysis of the records and accompanying introspections will be referred to again and there correlated with the later material.

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[1] The difficulty of making a separate record of these groups and their relation to the minute periods was so great, that it was considered best to transcribe them according to minute periods. This method gives us the chance to determine any possible relation the groups might have to the time record and also to the rate of the tapping curve. It does not materially obscure the actual error groups as they appear on the record irrespective of the minute periods.

c. Tests in which Exhaustion was Prominent.

In the simple series 4, 4, 2, 1, etc., subject 'Dy.' worked in the first practice test about fifteen minutes, reporting little or no fatigue, practically none in the region of the lip. The records were made by the lip movement described above. In the second series a week later, the digits 4, 3, 2, 1, were tapped. This simple series was given to allow the subject freedom from the disturbing influence of mental errors. After spending seven minutes in preliminary practice with numerous breaks and pauses, Dy. started in a natural, unemotional manner and continued for eight minutes when she stopped through exhaustion. This exhaustion was reported as general and it could not be located as specific fatigue of any one group of muscles or of any single organ in the body. It was expressed in psychical terms as a lack of any sort of imagery. The series of digits simply disappeared.

Where the taps could be counted the rates are about as follows: for second minute, 59 taps; seventh, 70; ninth, 70; thirteenth, 73; and for the thirty-six seconds just preceding the breakdown 53 taps were recorded, a rate per minute of 91 taps. There are very few errors throughout, on account of the simplicity of the pattern tapped. The breathing rate was as follows: 24.8, 25.2, 24, 22, 24, 22, 23, 22, 21.5, 23, 20, 18, per minute, showing a marked tendency to fluctuate and a gradual slowing to the end of the series. In addition to these changes, the average amplitude of the 48 respirations preceding the 'breakdown' is slightly more than twice as large as the average of the same number of respirations selected from the early part of the series. While becoming slower in the whole respiratory cycle, the respirations have distinctly deepened in both inspiratory and expiratory phases. The expiratory phase shows an increasing tendency to slight but distinctly marked irregularities.

The complex series was tried in two tests with this subject. In the first trial with the complicated series both the rate and the number of errors were kept practically constant till the break came. The rate per minute is as follows: 104, 100, 118, 110, 117, 112, 102, 110. The errors run 4, 6, 6, 6, 6, 5, 2, 5,—the drop in the seventh is due to repeating three of the simpler groups together by taking them out of their usual order. The rate of breathing runs as follows: 21, 21.5, 24, 20, 24, 19, 20.5, 18. This is an uncommonly high variation in rate and is practically the only indication on the objective side that the work was not well in hand. Introspectively, Dy. reported that she had just thought how well the whole tapping was going when suddenly and without warning the visual image of the series was lost and everything became blank, with no power to recover the imagery and continue the process. No muscular fatigue could be localized, but the heaviness that came with returning imagery of the situation was felt about the head. In the second test, 'Dy.' broke down completely at the end of fifteen minutes. She reported a

> "general tired feeling with considerable nervousness, a very noticeable tension throughout the body, a feeling of increased heart beat," and, as she stated it at the time, "all the phenomena accompanying a totally 'gone' feeling and lack of muscular control."

Another subject, 'Fl.,' succeeded in passing through the practice series but broke down repeatedly on the more extended test. The general description follows that given above in all essentials, though the introspections vary characteristically. A large amount of eye strain was noticed, and in the five tests this localization of strain and tension was reported first; the maintenance of the eye strain is reported as finally giving 'a general feeling of weariness.' In starting any single test, she reports a considerable tension about the head, neck, shoulders, and arms; this was gradually replaced by a relaxation that was irregular in order of disappearance, except that the strain of the forehead throughout the single experiment was replaced in attention at irregular intervals by the 'general feeling of weariness' mentioned above.

The results of these preliminary tests have been reported at length here since they indicate what becomes in many of the subjects, with this type of test, a frequent occurrence when any attempt is made to 'speed' them. We shall refer to the descriptions given here again in connection with the 'rapid series.' *These subjects were among the most skilful introspectionists of the group, yet their total failures come without warning and at times when introspection and the whole 'feel' of the process reported the work satisfactorly in progress. Exhaustion seems to be the result of a practically complete focalization of all available energy on the continuance of a single function.*

d. Group II. Results.

Rate of tapping.—The results of the experiments in group II will now be taken up. The majority of the rate records reported will be given on the basis of the number of taps per minute and per five-minute groups. A number of these records, however, present the same phenomena when counted in a slightly different manner and, when grouped by this latter method, are more easily correlated with the error groups; hence we shall mention certain results on this basis first.

This rate is determined by taking the total time necessary to record a single series of ten digits and its accompanying calculations, i. e., the time spent in repeating or tapping each 'cycle' of digits. The method[1] is inapplicable except in a few of the series that are freest from unambiguous errors and are entirely continuous. This method of dealing with the records, where used, finds results comparable to curves showing increasing automaticity, decrease in the time of doing any regular piece of work through practice, and all factors that go with the transition of a memory task, as it passes from the early stage of instability to practically automatic associations and mere 'cue' reactions.

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[1] One reason why this method of assembling the results need not be used throughout (when the rates are also determined on the scale of an arbitrary unit, as the minute or the two or five minute period) is that the two times will vary in parallel. A few determinations will show just how many of the arbitrary units, minute periods, approximate in length the actual average time spent in making the repetition of the whole problem or 'cycle,' and this average time when applied to the assembling of the rates will show the fluctuations that appear in the time spent in repeating this common unit of the series. As a matter of fact this precaution, even, is not necessary in order to show whether the practice gains and fatigue effects are opposing influences. Practically any grouping, from one minute up to five, shows changes parallel to the larger fluctuations in the time spent in repeating the common unit or cycle of the series.

The results in those series that could be counted thus are very interesting. They show, first, that the condition used, i. e., allowing the subject to select his own rate of speed, is as regular in the results produced as is the more usual one of insisting on the greatest possible rapidity. The decrease in time necessary to repeat all the digits memorized and to make the calculations is a fairly regular one, and where the actual time can be determined with accuracy the records show no leaps so large as to indicate that the subject is not in as perfect control of the process as if he were working at a maximum speed. The introspections that accompany the change in conditions are indicative of this. In the slow series the subjects arranged the mental speed to suit a rate of the lip that was easiest and readiest in getting the best results for any period of time. This means, of course, that they were not working as rapidly as in cases where they were asked to attain their speed limit, but it also means that the coördination of lip movement with memory images was a much closer one. All of the subjects when put on the rapid series become conscious immediately that they could go much faster if they could only make the lips move as rapidly as they saw, heard, or 'felt' the digits. It was necessary all the time to keep the mental 'movement' back in order to prevent tetanus and consequent confusion through the lip movements.

The following tables and curves will show the main results, and the comparisons that can be instituted at this point in the experiments.

Table II, a gives the number of seconds required to make the calculations and to repeat the entire forty digits constituting the cycle, or to make the 143 taps necessary in series 'B–1'[1] and the 130 of 'B–2.' The consecutive numbers at the left indicate the number of repetitions made in each case. The totals of the columns of seconds will give the number of seconds used in the several experiments. These are given for the con-

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[1] See page 55 for meaning of 'series B–1,' etc.

TABLE II, a.

SERIES	B – 1					C D F			B – 2						RAPID SERIES, D.	
SUBJECTS	Ch.	Rn.	Sd.	Pn.	Ym.		Ym.		Ch.	Rn.	Sd.	Pn.	Ym.	'A.'	Ch.	Ym.
TIME OF SERIES IN MIN.	25.5	20	27	25	23.5	16	21	28¾	22.5	20	25.5	21	23	21	20	19.5
Cycle 1	120	138	150	85	90	103	150	145	120	139	105	123	82	108	64	64
" 2	146	124	120	115	92	97	141	131	130	159	100	110	99	117	61	68
" 3	107	120	127	125	99	94	133	120	153	153	—	118	89	98	64	65
" 4	107	120	119	122	83	85	132	120	?96	?135	101	103	?71	89	59	69
" 5	120	128	117	119	84	90	137	108	123	152	99	101	98	89	58	59
" 6	110	107	109	?122	?42	78	137	117	119	150	76	110	94	86	69	60
" 7	112	115	101	132	?91	75	134	?72	127	147	94	119	95	83	63	58
" 8	110	100	117	129	84	80	129	108	?92	142	98	?101	87	84	68	?38
" 9	109	110	134	125	98	78	129	95	116		101	131	?59	80	?61	54
" 10	118	100	148	116	89	87		122	122		101	134	—	86	66	63
" 11	120		109	120	92	78		112	116		103		87	86	?56	62
" 12	123		106	116	81			101			91		81	83	60	59
" 13	110		113		78			99			82		?63	85	78	56
" 14					79			104			86		90	82	72	57
" 15					80			101			89		86		77	53
" 16					85						92		?60		56	54
" 17															61	53
" 18															62	54
" 19																57

venience of the reader at the top of each column, in minutes. The C, D, F, group assembles all the series of subject 'Ym.' which could be handled in this manner. Where omissions in the columns occur, the group was broken up in the record in such a way that it could not be determined exactly how much time ought to be counted as spent on the 143 or 130 digits though the actual total time could be easily determined by subtraction; since this was of little value without the size of the groups repeated or omitted, it is not given. Other blanks in the records or uncertainties of any kind are indicated by question marks placed with the number about which the uncertainty lingers.

All of the curves show the ordinary results of practice.

Practice Changes.—Outside a certain variation[1] due to the character of the task set, and entirely independent of it as is shown by the changes in the curve in the method here used, is a different grouping of the practice changes. For subject 'A.' the rise and fall in the curve is less prominent, because of two things. The practice series that had preceded this series had been but two in number as against the eight to ten of the others. Secondly, only one of the group could be evaluated in this manner, hence a lack of material. The results in the curves on the basis of minute periods are much like the groupings here shown. *The peculiar thing about the curves that was noticed first in the errors, and here only secondarily, is the definite decrease in the amount of work done or in the accuracy of that work at certain particular points in the single experiment.* Though they cannot be foretold to the minute, the general nature of the groups that will appear and where they will fall, remain singularly constant for the same type of work.

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[1] Practice changes are partly masked by the different degrees of difficulty presented by the work itself. For example, the repetition of the first ten in every cycle in the majority of the tests is that of a simple series of digits without any calculation. The second ten has calculation, and is thereby increased in size, so that when finally completed more actual taps have been made than in the first. This gives to the majority of the curves based on the amounts tapped per minute, a form that is slightly extraneous to the actual fatigue and practice effects of continuous work. The curves of group I, and 'A' of group II, show what the regular curve is as compared with those of the latter series.

Certain individual differences appear. In subject 'Pn.'s' records, the groups are larger and more noticeable toward the end of the series. These 'falls' are independent of the general rise or fall of the practice or fatigue curve, being superimposed upon it. In subject 'Ch.' they are more decided in character at the beginning and toward the close, control over the experiment being apparently better during the minutes ten to fourteen. With the exception of the initial drop of series 'B,' subject 'Rn.' shows the more steady portion of the series in the first eight or nine minutes, and after that the usual periodicity in rate. Subject 'Sd.' exhibits periods of decline in rate that are much slower in culmination than in any of the other subjects. The groups in the records of 'Ym.' and 'A.' are more numerous.

A second table to show the mean variations and averages of the times required to repeat the unit of the experiment follows. This is not intended as a mathematically accurate presentation, for the experiment and its data do not readily lend themselves to this kind of treatment.

The figures here given indicate somewhat more clearly than the original set of figures alone could do, that there are certain relations, frequently overlooked, existing between the parts of a single series of continuous mental operations. To show the variations for different subjects, the averages may be used to some advantage. The mean variations show slight increase in the 'at will' series over those which are done at the highest degree of effort applicable to this kind of work. Taking the plus and minus signs, we have the form of the practice curve excellently outlined. They also indicate the relative weight that can be assigned to practice after deductions are made, such as Anregung and Antrieb. They show further what is more important here, since general fatigue ought not to be very prominent in such short series, viz: the decided changes in rate of repetition that occur at fairly regular intervals. The discussion of individual variations will be omitted till the other results are enumerated.[1]

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[1] It may be appropriate at this point to repeat what has been said in regard to the work of Voss on a point somewhat similar to the one that comes out

TABLE II, b.

SERIES	B – 1					C D F			B – 2						R. S. – D.	
SUBJECTS	Ch.	Rn.	Sd.	Pn.	Ym.		Ym.		Ch.	Rn.	Sd.	Pn.	Ym.	A.	Ch.	Ym.
AVERAGE	116.4	116.2	120.7	119	87	86	135.8	113	125	149	94.5	115	89.8	89.7	63	59.1
A. V.	7.5	9.8	10.1	7.1	5.6	7.5	4.64	73.5	7.7	5.3	7	10	4.9	7.7	48.3	4.08
Cycle 1	3.6	21.8	29.3	−34	3	17	14.2	32	−5	−10	10.5	8	−7.8	18.3	1	4.9
" 2	29.6	7.8	− 0.7	− 4	5	11	5.2	18	5	10	5.5	− 5	9.2	27.3	−2	8.9
" 3	−9.4	3.8	6.3	6	12	8	−2.8	7	28	4	?	3	0.8	8.3	1	5.9
" 4	−9.4	3.8	− 1.7	3	−4	− 1	−3.8	7	?	?	6.5	−12	?	−0.7	−4	9.9
" 5	3.6	11.8	− 3.7	0	−3	4	1.2	−5	−2	3	4.5	−14	8.2	−0.7	−5	−0.1
" 6	−6.4	− 9.2	−11.7	3	4	− 8	1.2	4	−6	1	−18.5	− 5	4.2	−3.7	6	0.9
" 7	−4.4	− 1.2	−19.7	13	−3	−11	−1.8	?	2	− 2	− 0.5	4	5.2	−6.7	0	−1.1
" 8	−6.4	−16.2	− 3.7	10	11	− 6	−6.8	− 5	?	7	3.5	−14	−2.8	−5.7	5	?
" 9	−7.4	− 6.2	13.3	6	2	− 8	−6.8	−18	−9		6.5	16	?	−9.7	−2	−5.1
" 10	1.6	−16.2	27.3	− 3	5	1		9	−3		6.5	19	?	−3.7	3	3.9
" 11	3.6		−11.7	1	−6	− 8		− 1	−9		8.5		−2.8	−3.7	−7?	2.9
" 12	6.6		−14.7	− 3	−9			−12			− 3.5		−8.8	−6.7	−3	−0.1
" 13	−6.4		− 7.7		−8			−14			−12.5		?	−4.7	15	−3.1
" 14					−7			− 9			− 8.5		0.2	−7.7	9	−2.1
" 15					−2			−12			− 5.5		−3.8		14	−6.1
" 16											− 2.5		?		−7	−5.1
" 17															−2	−6.1
" 18															−1	−5.1
" 19																−2.1

Tables II, b, and II, c. The averages in the horizontal column are simply the arithmetical averages of the time in seconds required to repeat the cycle of digits once. 'Cycle' in this connection means the ten digits that are memorized plus the calculations that are imposed upon them at each repetition. The average variation is given to indicate with some degree of accuracy the amount of fluctuation that an 'at will' rate will occasion, and may be compared with the same in the rapid series given at the close of the table.

TABLE II, c.

SUBJECT	RATE CHANGE AS LOCATED IN.	SERIES B – 1.				SERIES B –2.				
A.....	Cycle					2		10–11		
	Min. period					3–4		15–16		
Pn....	Cycle	slight decrease		**6–7**	11		3	**7**	**9–10**	
	Min. period			12–14	22		5–6	13	17–19	
Sd....	Cycle		3		**8–9**		**4**	9–10		**15–16**
	Min. period		6		16–19		5	13–16		23–24
Rn ...	Cycle		**5**	**7**	**9**	2		**5–6**		
	Min. period		9–10	13	18–19	3–4		11–13		
Ch ...	Cycle	slight decrease	**5**		**10–12**		3	**7**	**10**	
	Min. period		9–10		19–23		6	14–15	19–21	
Ym...	Cycle	3	7	**9**		2	5	**9–10**	13–14	
	Min. period	4–5	9	12–13		2	6–7	13–14	18–20	

Table II, c, shows where the distinct decreases in the rate of tapping appear. Those in dark-faced type are the more important of the rate changes; they are located in the whole test in two ways, first by the 'cycle' and secondly by the minute period where the change occurs.

So far our results with one important exception are entirely comparable with those of other investigators. We have found practically all the individual variations that have come out in earlier experiments. The practice curve in its various forms occurs ranging from the curve showing continuous practice effects during the time spent in the test to the opposite curve of continuous fatigue, the negative practice curve.[1] The curves show distinct evidences of Anregung, Antrieb, Ermüdungsantrieb.

We attempted, by careful questioning, to determine the subjective nature of the rate curve during these experiments through the introspections of the individual subjects. In no case in the 'at will' tests could we elicit any significant answer to our question about changes in the rate where positive introspections were obtained. The records show that there was as much chance of an opposite change actually having taken place as the one assigned by introspection. Some of the subjects even went so far in one or two instances as to state that they were

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here, though the main similarity comes to the front in the terms he has seen fit to apply to the variations that he found in mental work, rather than in the nature of the results themselves. Using the additions of the Kraepelin method of experimentation and measuring the length of time necessary to make each separate addition he found an exceedingly regular series of fluctuations. The 'mode' of the curve fell at lengths of 13/5 seconds, and the sides of the curve fell off in the following frequencies: 7/5, 10/5, 16/5, 19/5, 23/5 seconds. These are, according to the author, entirely comparable to the fluctuations of attention found by many other investigators in the field of sensations. He concludes that he is dealing with the same phenomenon, and explains it as a central one. In so far as the experiments we have carried out are amenable to this method of treatment our results agree with astonishing accuracy with these details published by Voss. The work we have set our subjects is much more difficult, but otherwise the results are similar. Voss made no attempt to evaluate the larger fluctuations that we mention as the most characteristic thing in our curves. *A priori*, we would expect to find them smaller in the simpler work; he merely mentions that such fluctuations were found in rate and that they seemed to show no regularity. We shall find the same thing true in the method of handling the errors in the more homogeneous type of work set by these experimenters. Their work and method of experimenting do not bring out the larger changes in attention which may be correlated with 'synaptic' fatigue.

[1] To be sure, this is merely a happy accident as far as the experiment planned is concerned; it could not be foretold that such was going to be the case in the selection of the subjects.

conscious of following the motor in its regular whirr, although the records in these two or three cases are particularly definite regarding the fatigue impulse. Other changes, as the lessening of calculation pauses, averaging up the intervals preceding easy and difficult calculations, and the like, correspond without need of comment to such results as those of Voss and others.

The one exception noted in our results we find in the distinct groups that occur, which we have described above. The complexity of the work is, no doubt, partly responsible for their appearance. Still they have been clear enough for previous mention by Voss and, in a number of the tests carried out on school children, their prominence has been noted, but no attempt has been made actually to describe them. We are undoubtedly dealing here with the 'Ermüdungsantrieb' that Kraepelin has described and named as the spurt that comes near the end of a decrease in the rate of work. The curves here shown, after the practice phases of the curve are taken into account, indicate that the time spent in the decline is always longer than the time spent in the recovery, hence Kraepelin is justified in calling such rapid increases in the rate 'Ermüdungsantrieb.' *But the significant part of the curve for fatigue is the slow decline of from two to three minutes in length that occurs at fairly regular periods.* Kraepelin has mentioned but one variation of this type, that one which precedes the declining portion of the work curve and comes, in the two hour test, at a period ranging from 70 to 90 minutes after the beginning of the experiment. The numerous Störungsantriebe that, so far as introspection and the form of the record are concerned are entirely similar to the 'Ermüdungsantrieb,' Kraepelin considers too irregular for evaluation.

It is possible to find the changes of which we speak in those records that were done at the limit of speed, though they are lessened in degree as may be expected. With work that is much simpler than our own and more mechanical in its method of record, it seems possible that these fluctuations will almost entirely disappear from records as gross as these. The work on muscular strains and the relations of their regularity to the relative weight of the loads lifted indicates what we mean by work tests where fluctuations disappear. A point has been reached where practice as well as fatigue disappears, and this point constitutes the zero point for the coördination of the individual with reference to a specific kind of work, mental or muscular. In other words, the habituation process goes on below the level of objective fatigue.

Errors.—In the tables that follow, the results of the experiment are presented from the standpoint of the errors that appeared. Their grouping, their increase or decrease in a single record, and their possible classification, will be discussed.

The absolute number of errors increases very slightly in some records, from one minute to the next; the reverse statement is, however, apparent in others. There is an initial group of errors due to lack of practice. This initial group soon drops to a minimum or to no errors per minute. From this point on the errors in some records gradually increase in number. This is true of a majority of the tests. Certain records, however, present an increase after this initial group, then a slight decrease.

The interesting fact that appears in regard to the errors, however, is the fluctuating character of the totals per minute. This has occurred in every series so far recorded. In any one subject the errors tend to group themselves. These groups occur at more or less regular intervals for a single subject. The length of time between the intervals varies apparently with the amount of practice and the degree of difficulty of the particular series. In some instances, instead of the actual number of the errors in any group increasing, they may slightly decrease. But as long as errors appear in the record they tend to group themselves. In other words, the errors, which occur after the first gathering of the sensory controls that are necessary in order that the process may go on at all, are more or less constant and independent of the real results of practice.

Grouping in 'practiced 'series.—After the errors begin to appear, even in a practiced series, that is, when the smoothness of the mental 'coördinatings' is overcome by the preliminary stage of fatigue, then grouping appears in the manner described and it is in no specific sense different from that form in which

TABLE III, a.

Subject	Ch.							Sd.						
Series	A	B – 1	B – 2	C	D	E	F	A	B – 1	B – 2	C	D	E	F
Min. 1	0	2	0	0	0	0	2	0	1	0	0	0	0	0
" 2	0	4	3	0	0	0	1	0	1	0	0	0	0	2
" 3	0	2	0	0	1	0	0	0	2	0	1	1	0	0
" 4	1	0	0	0	2	0	1	0	1	5	0	0	2	1
" 5	0	1	0	0	2	0	1	1	1	1	0	1	0	0
" 6	2	0	0	0	3	1	1	0	2	0	1	4	1	0
" 7	0	2	0	0	2	0	1	0	3	3	1	3	1	0
" 8	0	2	2	0	2	2	0	0	1	0	0	2	2	0
" 9	0	0	3	1	3	3	1	0	0	1	2	1	2	1
" 10	1	0	0	0	0	0	1	0	0	0	5	1	2	1
" 11	0	2	0	1	1	0	0	0	1	0	0	0	5	0
" 12	0	0	1	1	2	2	1	0	1	1	1	2	1	0
" 13	0	2	1	0	2	1	1	2	3	3	1	0	1	0
" 14	2	3	0	1	2	0	1	0	0	1	0	0	2	1
" 15	2	3	2	0	1	0	2	0	4	0	3	3	4	1
" 16	0	0	1	1	0	0	0	1	1	4	1	2	3	0
" 17	1	0	1	3	0	0	0	0	2	5	0	0	2	0
" 18	2	1	1	0	0	0	0	0	1	4	3	3	1	6
" 19	1	1	0	0	0	0	0	0	2	1	2	1	4	0
" 20	1	0	1	0	0	3	0	0	4	0	1	1	4	0
" 21	0	3	0	0	0		0	6	1	1	1	3	3	0
" 22	1	0	3	0	0		1		1	2	2	0	2	2
" 23	0	1		0	0		0		2	1	4	1	3	2
" 24		2		1	0		0		2	2	3	4	2	0
" 25		0		0	0		0		5	0	4	3	5	1
" 26				3	0		1		3					
" 27									0					
" 28														

Table III, a, is a detail table taken from the work of two subjects. In general they represent records that indicate an average grouping or one slightly below the average. Cross lines in the column indicate where the word 'change' spoken by the operator introduced the new part of the record; see p. 56.

it showed itself in the new series or any repetition of the series. The use of the new or 'practiced' coördination within the working period of a test, as in series 'D,' etc., is a decided relief as far as the making of errors is concerned. The relief is also evident when a new series is followed by another series

TABLE III, b.

SUBJECT	A.							Pn.							Sd.							Rn.							Ch.							Ym.						
SERIES	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C	D	E	F	A	B–1	B–2	C	D	E	F
Min. 1			5			3																																5	3			
“ 2							4														2		5				3			8	3				3		5	4		3	3	
“ 3	5	5		5	15?											4		1								2										4		4				19?
“ 4									2	4	5	5	2	4			6			2				6				5										4	8	6		
“ 5											3				1								3														12	3				
“ 6				6			7							14?					9							2			3				7		4			5		5	3	
“ 7																6	4						5							4						5		6				
“ 8	3				3				5	7		5								5								6										3				9
“ 9							2			7			3	5							2		5	7		2					5		7	5		6	8	1	23	12		
“ 10																		7				2	5					6										5				
“ 11		5	5			3?						4								8							2			2		3						2				
“ 12				4									5	4					2							2		9						3				3			3	
“ 13	8						4		2		5				2	5	5					2			6								6			10	8	8				14
“ 14			4											5							2			2		2				8								5	24	12		
“ 15	9			3							3	4	5			7		4	5	9			6				2		4		3				4			4				
“ 16						2?									1										8													1			5	
“ 17										5				6			13					6	3					8				4					7	7	4	4		14
“ 18					3							4	4					6	4		6								4									4			3	
“ 19			3				4	3			4			4						9				5			5											5				
“ 20																7									2									3			10	2		10		
“ 21															6				4	5		3		6				6		4						4		4				
“ 22																	5				4										4				1			2				
“ 23																		9																			7	4				
“ 24																			8	10										3												
“ 25																10																										
“ 26																																4			1							

Table III, b. This table gives in totals the errors of each test for the six subjects who made the entire series of tests. We may illustrate the arrangement by reference to the errors made by subject A, series ‘A’. The errors did not occur exactly as shown but are massed at distribution points. The place where the figure is min. 3, 8, 13, etc., denotes the minute containing the largest number of errors. The minute periods before and after this one always contain less errors. A comparison of this table and Table IV, a will show how it is made up. Turning to Table IV, will enable one to test the validity of the grouping shown here. The errors of Ym. for test B–2 are given in full to show the series in which grouping is least apparent. Reference may be made to pp. 56, 57 to determine the nature of each of the series marked A, B–1, etc.

that is new, or a highly practiced series is followed by one that is new. The relief here cannot always be determined from the record because of the demand for new coördinations.

Arrangement of groups.—The distance between any two groups is more or less constant for different places in the series. There are seldom long periods wholly free from errors. This distance is practically always greater between the initial group and the first group. (i. e., the first group that seems to be definitely due to the effects of fatigue and the wanderings of attention) than it is between any of the groups that follow. The instances where this is not exactly true will be discussed separately.

There is no general tendency for the errors to increase in their absolute number as the work of the series progresses beyond the first fifteen or sixteen minutes of the series. Exhaustion or sleep is the new phenomenon that appears rather than any quantitative change. As stated above there is in the majority of the single experiments no rate change in this short experiment that would indicate the rise of general or lasting fatigue.[1]

Tables of Grouping.—It is somewhat difficult to determine just the manner in which these groups in their details ought to be measured, either to show that they are of less importance than they at first appear to be, or to bring out their real nature with respect to the other characteristics of the work curve. In order that there shall be as little danger as possible of 'forcing' any of the changes or of covering up others that ought to be brought out, we shall try several methods of assembling the results. The first in order of presentation is that which we have mentioned, (Table III, a), (1) the simple arrangement of the errors as they occur by minute periods in each continuous series. After that we shall (2) group in order the errors as they appear to arrange themselves around certain points in the series (Table III, b), and (3) as they appear when brought together by two, three, four, and five minute periods (Table III, c and III, d). The actual distribution in the tests, which is practically similar to the minute periods, is omitted.

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[1] In this connection it seems advisable to state again that there is no intention to institute a comparison beyond the more general features between these records and the two-hour and longer records of other investigators. What would happen to our curves if arranged with that end in view is in its general features undoubtedly what has been pointed out before, but we should expect to find accompanying those phenomena the others just mentioned.

TABLE III, c.

	TOTALS BY TWO MINUTE PERIODS.							TOTALS BY THREE MINUTE PERIODS.							
SERIES.	A	B – 1	B – 2	C	D	E	F	A	B – 1	B – 2	C	D	E	F	
Min. 2.....	0	6	3	0	0	0	3								
								0	8	3	0	1	0	3	3 Min.
" 4.....	1	2	0	0	3	0	1								
" 6.....	2	1	0	0	5	1	2	3	1	0	0	7	1	3	6 "
" 8.....	0	4	2	0	4	2	1								
								0	4	5	1	7	5	2	9 "
" 10.....	1	0	3	1	3	3	2								
" 12.....	0	2	1	2	3	2	1	1	2	1	2	3	2	2	12 "
" 14.....	2	5	1	1	4	1	2								
								4	8	3	1	5	1	4	15 "
" 16.....	2	3	3	1	1	0	2								
" 18.....	3	1	2	3	0	0	0	3	1	3	4	0	0	0	18 "
" 20.....	2	1	1	0	0	3	0								
								2	4	1	0	0	3	0	21 "
" 22.....	1	3	3	0	0		1								

Table III, c. The above table exhibits the errors as they occur in the tests made by subject Ch. by two and three minute groups. These details are given as representative of the tests for all the subjects and may be compared with those tables where the errors are grouped according to their apparent distribution as in the preceding table.

Location and number of groups.—We find an initial group in practically all the series. There are two exceptions to this. The first is in those series, marked 'A,' where the subjects had had a preliminary practice series on the actual ten digits used here, of eight to ten days. The second is where the material given proved to be less difficult for the subject than expected, because of some special method of learning the material. *This first group appears within the first three minutes of the test, and with an exception to be noted below, at the end of the sixth minute is approximately over.* This initial group is

TABLE III, d.

	PERIODS.	TWO-MINUTE GROUPS.						PERIODS.	THREE-MINUTE GROUPS.						PERIODS.	FOUR-MINUTE GROUPS.						PERIODS.	FIVE-MINUTE GROUPS.					
SUBJECTS.		A.	Pn.	Sd.	Rn.	Ch.	Ym.		A.	Pn.	Sd.	Rn.	Ch.	Ym.		A.	Pn.	Sd.	Rn.	Ch.	Ym.		A.	Pn.	Sd.	Rn.	Ch.	Ym.
	2	25	13	4	12	12	34																					
								3	43	23	8	22	15	50														
	4	21	15	12	9	7	35								4	47	28	19	27	19	69							
																						5	48	39	21	32	23	79
	6	16	20	11	11	11	34	6	20	25	21	17	15	53														
	8	8	18	14	17	13	37								8	24	40	28	32	24	72							
								9	14	26	23	28	24	60														
	10	9	13	9	17	13	45															10	28	42	40	44	34	88
	12	20	19	11	17	11	33	12	22	26	21	27	13	55	12	29	32	28	36	24	77							
	14	15	15	13	18	16	51																					
								15	26	21	29	31	26	67								15	47	40	41	49	38	90
	16	13	15	23	18	12	35								16	30	29	41	39	28	86							
	18		16	24	13	9	34	18		25	39	21	11	‘53’														
	20		7	17	16	7	‘23’								20		23	47	29	16	‘57’	20		34	65	36	18	‘69’
								21		‘8’	35	19	10	‘42’														
	22			21		8	‘29’																					
	24			‘19’		4	‘17’	24			35		‘9’	‘27’	24			50		‘12’	‘46’							

Table III, d. This table gives the *total* errors for six subjects for the seven tests constituting group two (II) of the experiments. By reading down the columns any increase or decrease in number of errors may be readily noted. The ‘Periods’ are serial; the numbers below the word ‘Period’ merely indicate the last minute of that period.

undoubtedly due to the unpracticed character of the work and the newness of the calculations to be made.

After the first or initial practice group, the record usually shows a decided drop to no, or relatively few errors, till a point approximately covered by the period from the ninth to the eleventh minutes. This second group is pushed on by the initial period coming farther along in the series, when it may fall as late as the twelve to fifteen minute period. The time and place in the series are not absolutely determinable but the groups are there in every case. An extension of the group from the sixth to the tenth minutes would include all of the error changes that are significant and seem to be due to causes other than those operating through lack of practice and the beginning inertia.

We find this second group sometimes coming at the beginning of the period mentioned, at other times at the close of it. If the first condition exists, then the third group will fail anywhere from ten to thirteen minutes inclusive. If the latter condition prevails, the third group is pushed on to the thirteen and fourteen minute period or even as far as the fifteen to sixteen minute period. In any case, where the grouping has appeared at all in the series in the places above described, a third group always appears by the close of the sixteenth minute and usually before this. This group as well as the second will be of especial interest, in that it is here that the indications of the physical and mental condition of the subject come to light most prominently.

When the experiment was carried on far enough, a fourth group appeared in the period ranging from seventeen to twenty-one minutes. This wide limit is given, not because any one series shows the group extending over this period, but because of the few experiments made. Usually this group has been the most definite and regular after the third that has appeared, when the series has been long enough to include it. It was somewhat unfortunate that they were not made of such a length as always to include this, but nothing of the sort was expected in the inception of the experiment.

The distance maintained between these groups is subject to constant fluctuations; which, to the writer, indicates the really psychological significance of the groups that do appear. The groups are too gross and too irregular to indicate any closer correlation with the fluctuations that are commonly assigned to the attention process and probably they show none at all. Their origin and place in the series must be explained on other grounds.

This grouping of errors holds true even after the practice has been carried to a high degree of automaticity. Groups of the later orders could be found even after the eighth and ninth days, in the practice series. These correspond fairly well with the periods of the less practiced tests when the changes due to practice are considered. It is found that retention of practice (Uebungsspur) will affect this type of error and work though in much less degree than is popularly supposed. Swift (81a) has shown what some of these experiments indicate, that the errors remain constant or even increase while daily practice effects are still going on. This increase in errors is, however, a common phenomenon in tests on school children and appears to be independent of the practice change as that appears in an increase in the amount of work done. How the practice does finally affect the errors by reducing them, has never been discussed so far as the writer is aware.

Pivotal Groups.—If we discuss the errors quantitatively, we find one or two things that seem worthy of note. For example, group four of the errors in all the series shows a constant tendency to drop below, in the number of errors, the preceding group; i, e., *group three is practically always the largest group in the series.* This is a constant phenomenon if we except the results of the subject 'A.', and take the totals of the errors by five minute or four minute periods. The preliminary group in the case of 'A'.s' record shows the largest total of errors, due no doubt to the lack of preliminary practice. This is borne out by the practice curves in the records of this subject, which are very pronounced. Subject 'Pn.' shows a change that is quite parallel with his negative practice curve. His *largest group of errors,* counting by five minute periods still falls in the second group instead of the third as usual. This change is interesting when we consider that he is the only subject of the number used, who presents a consistent negative practice curve.

In several instances, where the record was run up to twenty-eight and twenty-nine minutes before stopping, a fifth group appears, showing that the tendency to group is not lost after group four is over. Group five is best exemplified by the records of 'S.'

As far as can be determined, and the introspections will be discussed in this connection later, the number of errors and the distance between the groups, their size and length, their place in the series, all depend on the actual physiological condition of the subject as he begins the experiment, rather than on any particular degree of practice attained or on the difficulty of the work or on any shift in the conscious processes. Some few shifts noted above may be considered as exceptions to this statement: special imagery disturbances, external distracting stimuli, etc.; but they are exceptions which in no wise seem to affect the general statement here made.

When the errors appear constantly, or in much greater proportions than usual, the introspections tend to substantiate this by presenting some condition that could not be avoided except by interrupting the continuity of the series. For example, 'Ym.' shows the constant effects of a lowered physical condition in which he was compelled to record the series. 'Pn.' also shows the necessity for a particular type of control in the negative practice curve. His records were taken at the ebb in a difficult summer's work. The distance between groups of errors seems to be particularly instructive with regard to the general health under which the different subjects performed their tasks. Those working under poor health conditions show a very marked tendency to make the groupings shorter, more frequent and less clear cut, then the groupings that occur in the records that are made under favorable health conditions. As an illustration, the contrast between the groups in the records made by the two subjects 'Sd.' and 'Ym.' is extreme as regards the number of errors and the distance in time between the error groups.

Groups Two and Three.—A very interesting point appears in connection with the relation of groups two and three. The relation of these *two groups* is most clearly shown, though by no means obscured in other tables, when we count errors by periods of five minutes each. If we recall that group three is the largest group of the series, we see at once that group two is usually next in size. Not only is it the next largest group, but it also bears much closer relationship to the following group than is shown by any other pairing that can be made. It pushes into it and modifies it continually in such ways that the third group could not be described without reference to this first appearance of the errors after practice control had been gained over the series. Where a group disappears, it is one of these, or a coalescence of the two.

Evidence of the great fatigue importance of this point is offered in the high speed series that were instituted as a control over the regular experiments. Two of the subjects, neither of whom had had any difficulty with the other series, when asked to try this, were compelled to stop at the end of eight and a half or nine minutes, through tetanus of the lip and especially a general loss of muscular control. In the subject 'Ch.' tetanus was the principal annoyance; there was no control to 'show how and which digits had been counted.' Quoting further from the introspections, 'Ch.' says that "toward the close, sympathetic movements of the fingers and toes and other parts of the body were noticed." 'Rn.' succeeded in passing this point on the first record by stopping at intervals, but on the second day's work had to stop entirely because of the general loss of muscular control. This was shown by involuntary jerking movements of the arms, legs, and smaller paired muscles throughout the body, there being no apparent favor shown any one muscular group over another. 'Ch.' reported that after she had stopped, the lip and surrounding muscles seemed in better condition to continue than a great many other muscles that were affected.

The first two subjects mentioned above, whose records are reported as exhaustion groups, in two of the experiments failed twice each to carry the series over the nine-minute period.

The last test with 'Dy.' left her in such a condition that no more experiments were tried for some weeks.

Periods in which the subjects forgot the place in the series completely and had to be assisted by the experimenter, or stopped till the place was found, occur in the other records. These failures were well scattered and do not constitute anything distinctly comparable to the conditions above described.

Nature of errors.—An attempt to measure the character of the errors was made. This is largely unsatisfactory because of the reasons so often advanced against any such efforts. These reasons have been argued against the work of Sikorski, Ebbinghaus, Ritter, Burgerstein, and others, so only one or two main points will be restated.

In the first place, it is argued, it is impossible to locate the introspections on the record even if they be perfectly definite, and not interrupt the continuity of that record. Unless this is done there is no means of telling what the majority of the errors mean in mental terms, except where the coincidence is accidental. The calculation of what is an error and what value shall be given to the different types that can be distinguished in the records, are difficult points. This argument is complicated by the further observation that one error of the graver sort will have in its train several of less importance, whose actual importance is thus materially lessened. The question of including extra long periods of waiting between the separate groups as errors and causes of errors is pertinent, if one argue that all changes must be evaluated from the error standpoint.

In the tables appended, all of the observations mentioned operate, though we have given none a determinate value in measuring the number of errors. The reason for this appears to best advantage in the tables themselves. Everything that was not correct was marked an error in the tables first made from the records. These were used as the basis for our discussion concerning the grouping of errors, their increase or stability in number throughout the continuous series, and in the other discussions. In order to be sure that all this would not be vitiated by the possibility of one error constituting the whole of the group phenomenon described, and for the other purposes here stated, this attempt to classify was made. The classification takes account therefore only of those errors that could be placed, and attempts to so define the types that there shall not be left a large number of errors unclassified. (The whole number of errors in these tables is equal to the whole number of errors found, so that all the errors are accounted for that are found in the discussion of their grouping, or that were marked as errors at any time.)

The following classification was followed throughout the study of the errors; (1) omissions of digits constituted one set of errors; (2) repetition, a second; (3) tapping more or less than the requisite number of strokes, where the more or less was only one stroke, constituted a third set; (4) mental uncertainty, i. e., the irregular form of tapping that could not be due wholly to an irregular lip movement; (5) splitting the digit into two smaller ones, practically the same mental error as the above, but entirely definite and clear as to what it means, viz: a pause in the mental process, then recovery, and finally finishing work consciously incomplete; (6) reversing two or more digits in a series, as 2, 4, for 4, 2, and 2, 3, 4, for 4, 3, 2; (7) errors in calculation, where they could be distinguished from class (3), the criterion being a digit that differed from the right one by two or more taps, as 6 for 4, or 7 for 9; (8) tapping a digit from another series than the one being tapped, where it could be distinguished from other errors, was counted as a separate group; this determination was possible only where a group of two or more digits was taken bodily from some other series, otherwise the error was assigned to the calculation and uncontrolled tapping classes. Error type (3) is divided into two parts in the tables, one for the surplus tapping and one for the failure to tap the complete number. The omission, repetition, and calculation classes were subdivided to show whether the error covered one digit, a group of digits within the series of ten, or the entire ten digits; for example, omissions were marked as follows: O_1, O_2, O_3, etc. (This table is not appended in all its detail.)

The first table constructed showed the character and number of errors for each of the forty-two experiments in this part of the problem. These were then reduced to the figures representing the totals of each type per minute instead of the letters and characters used to designate the nature of the error and its place in the classification. This last table is the one given here.

Examining this table, we note that each type of error separately shows the distribution into groups and a distinct periodicity of a nature quite similar to the totals, but still differing enough to make it a class by itself. The totals of the entire sum of errors were then tried since this grouping appeared so well; and again, although the series used differ considerably in the arrangement of the experiments in several of the series, as well as in the changes made at irregular intervals in the series in some of the experiments, the totals are still found presenting a slight tendency to carry out the grouping first described. This is best illustrated by the average variations, or by a curve based on the totals of the fluctuations. To the writer this came as a distinct corroboration of the first table since it was not expected *a priori* on account of the great variability of the groups and the actual lack of practice effects of a positive sort in one subject.[1]

Two general classes of errors.—The errors as they are classsified seem to break up into two sorts, those that are due to so-called central causes, or purely psychical processes, as memory, vagueness of imagery, distinct stoppage of the thought process, etc., and those due to the more immediate sensory-motor process of recording. In the first class, we can easily put such errors as repetitions, omissions, reversing digits,

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[1] Regarding the error groups of Table IV, it may be mentioned that the calculation errors are not so numerous as might be expected from the difficulty of the work. This may be due to two things: the difficulty of determining from the record what actually is a calculation error, and second, the way in which the experiments were arranged, i. e., giving the subject a chance to select his own rate which enabled him after the practice series to hold a rate that brought inside itself the work done on the calculations. The first cause undoubtedly operated in counting the errors and distributing them for subject 'A' into the divisions marked calculation and uncertainty errors respectively. We have put practically all the errors in the last division and have been unable to place any with certainty in the former divisions.

TABLE IV.

TYPE OF ERRORS.	a. REPETITIONS.						b. OMISSIONS.						c. EXCESS IN NO. OF TAPS.						d. INSUFFICIENT NO. OF TAPS.						e. TAPPING UNCERTAIN.						f. SPLITTING DIGITS.						g. REVERSED DIGITS.						h. CALCULATION ERRORS.						j. DIGITS FROM ANOTHER SERIESTAPPED.						TOTALS.	Av. & A. V.
SUBJECTS.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.	A.	Pn.	Sd.	Ym.	Rn.	Ch.		49.
Min. 1	1		2	1	1	2	3	1		1	2			1		7	1			2		4	2	1		1		2	1		2	1		1	2		1			1		1				8									53	+4
“ 2	1						5		1	1	1	2	1	4		8	2	2				4								1	1			2		3											1					1	1	1	43	−6
“ 3	1			1			1	2			1	1		2	1	6		1	2	1	1		2	1	1	1	2	1	1		1	5	1		1											2						2			42	−7
“ 4						1			1							8	1	2					1		1	1		1	1		3	1	4	2	2	1				1					1	1		1					1		36	−13
“ 5		1				1					1	2	2	2	3	6	3	1	1	2	1		1	1	2	1						2	1	3	1					1															39	−10
“ 6		1	1	2					2	1			3	1	2	6	4	4	1	1				2	2		2			1	6?	4	2	3	1				1								1								54	+5
“ 7		1					1	1		3	1	1		4	5	8	2	1	1	3	2			1	2	1	1		1			2	1	3					1							2	2	2							53	+4
“ 8			1			1		2		1		2		2	2	10	2	4		1		2	2		5	1			2	1		2	2	1	1	1										3	4								55	+6
“ 9	3		2	1						2	2	2			3	6	5					5	1	5	2	3	1	1	2	1		4		3		2								1			1								58	+9
“ 10		3	3		1		1		2	2	2		1	1	1	4	3	1		1	2	4			2	1		1			2	2	1	4	1	1					1					3	3						1		55	+6
“ 11	1		3	1			2		2				2	3	1	12			1		1	1	1	1	2				2		1	1	1	2		1					1	1				3	3								50	+1
“ 12	1	1	1	1	1		2	1		1	1	1	1		3	7	3	1	1	1			1	1	2				1	1		2	1			2		1			1					2		1							44	−5
“ 13	3			1	1	1		1			1	1	1	2	1	7	8				4	3	1	3	1	2				2				2	2				1						2										51	+2
“ 14			1	1	1	2	1			2		1		3	3	5	1	1	3	1	1	4	1	1	3	2		1	1	3	1	2		2	2	1										4	1								56	+7
“ 15	2		2	2	1			1	3			1	2	1	3	8	2	2	1		1	3	2		5	3			1	3		2	2	2		2				1						9		1							68	+19
“ 16	1	1			1	1		2	3	1		1	2	2	4	8	4					5	1	1	3	1	1		1					1	2										2	1									50	+1
“ 17		1	3	2	1		1		3	2	1			2	2	7	1	1				3		2		3				1		2		5	2			1								3						1			50	+1
“ 18		4					2		2				1	2	5	5	1	2			2	1	2	2			1		3				2	2		2					1				1	5									48	−1
“ 19		1		1			1	1	6	1	1		3	1	1	5	2		2	1	2	2		2	2	1					1				1										2	2						2	1		45	−4
“ 20				2	1			1			2	1				2		2		1						1				2		1		3	3					1	1					1	1									
“ 21					1	1			7	1		1			10						3									3				1						2						4						1	1			
“ 22		1	1			1						1			9	2						2					4			1			8	1		1										1							1			

tapping those from other series, and the calculations. In the second class fall those where the number of taps made falls short of, or is more than, the requisite number by one, and where mere uncertainty is indicated in the way the digit is tapped. The division where the digits are split into smaller ones is hard to place. It is possible that its errors fall in classes already given, and, if we could determine the facts in the case each time, the entire division might divide into either or both of the others. It at least tends to take on some of the characters of both classes, rather than a third form.

Possible origin of errors.—All classes of errors appear in the first group and all the subjects' records except 'Sd.' This first group cannot be assigned to fatigue effects, monotony, ennui, weariness or any of the causes that are found to operate at points in mental work where the subject is popularly supposed to be at a disadvantage in the amount and accuracy of the work turned out because of the previous work. We may conclude in tasks as difficult as those set here that the errors in the beginning of the series are caused by this severity of the task, the lack of practice and all that this lack of practice implies in the formation of more or less new sensory-motor coördinations, in the incompleteness of the memory process, and the inertia of beginning a new task of any kind. The subject does not possess the control over the process in the beginning that he later comes to have—it is highly conscious, as over against the fairly automatic character that it finally assumes. The fundamental motor coördinations and attitudes that shall enter into the process must be gathered from the tasks of the previous hour, or from a resting period, and brought under the dominance of this particular task. The new sensory-motor coördination must be related to these more fundamental and permanent motor relations.

The first part of the psychological task is to establish this preliminary control. How much of the energy of the body as measured in the number and intensity of strain conditions of the muscles, shall be used in the present task? This question must be answered independently of the immediate sensory-motor coördination of the recording part of the motor mechanism. Upon this more deeply lying motor organization also rests the possibility of the retention of practice gains and the regularity of the process as a whole.

This does not deny the necessity of the 'external' motor process and its important rôle in practice and fatigue. Reserve adjustments of this second type may remain idle throughout the ordinary psychological experiment. In such cases the retention of practice is precarious, frequently impossible, and always ephemeral. It is only as the practice effects of the single test, increased rate and ease of working, become enmeshed in the relations existing within this deeper lying coördination that we reach the stage of firm practice retention and the level of skill. When the adjustment is momentarily satisfactory the task is not yet complete. There are continual trial and error processes going on in the effort to select some attitude that shall suit the situation still better and require a smaller expenditure of energy in accomplishing the task. This is in general the problem that must be met in the beginning of the series; hence the probability of all kinds of errors entering into this group is quite high. An organized coördination that will carry the task for a time is easily picked up in the majority of cases (as is shown by the rapid diminution of errors and the short period of comparatively few errors that soon appears) but is never lasting.

The errors that drop out first show this; they belong to the group that finds its origin in the more central processes, as they are usually called, and belong to what Sikorski called 'psychic errors.' Parts a, b, g, h, and j of Table V show the speedy diminution of errors of this sort, our class I. The other parts of the table, c, d, e, and f, continue to appear as errors in a single test somewhat longer and even alternate with these 'psychic errors' in several places. The main characteristic is, in the case of the mechanical errors, a tendency to spread along in the series as a more constant phenomenon than the others. This is not wholly true in individual cases. But, if we refer to their alternating character and their tendency to scatter along the series as accompanying phenomena, we will find it possible to distinguish in every case between the two classes

TABLE V.

MINUTES	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Class I.																				
Totals	25	16	11	6	5	9	15	13	14	22	17	16	11	10	23	15	19	17	15	11
A. V.	10.5	1.5	−3.5	−8.5	−9.5	−5.5	0.5	−1.5	−0.5	7.5	2.5	1.5	−3.5	−4.5	8.5	0.5	4.5	2.5	0.5	−3.5
Pos. or Neg.	+	+	−	−	−	−	+	−	−	+	+	+	−	−	+	+	+	+	+	−
Class II.																				
Totals	27	27	33	28	31	41	35	38	41	29	32	28	32	44	42	34	28	32	26	15
A. V.	−5	−5	1	−4	−1	9	3	6	9	−3	0	−4	0	12	10	2	−4	0	−6	−17
Pos. or Neg.	−	−	+	−	−	+	+	+	+	−	0	−	0	+	+	+	−	0	−	−

Table V. This table gives the totals of the two classes of errors, to show the difference in distribution. Besides the totals are given for convenience, the average variation and a column of + and − signs to indicate on which side the deviation is and how much this deviation amounts to in each minute of the tests. The totals do not exactly correspond to the whole number of errors as given in the preceding table, Table IV; errors that could not be placed at all were omitted. The detailed tables that were made out and of which this is an abstract show exactly the same phenomenon as the above, but more clearly since the individual variations in the distribution of the errors is not lost.

of errors. Class I (See Table V), distinctly tends to omit what we have called group two in the errors and begins toward the close of that group and runs over into group three with considerable strength. This can be shown by summing the errors for individual subjects by classes, when for subject 'A,' for example, after the first group is passed, there is almost a total lack of errors of class I, until the ninth minute, while in the minutes from five to eight this first class of errors does not appear at all.

Certain variations in the process were introduced in some of the experiments. These are shown by the horizontal lines in Table IIIa, and the other tables. Their nature was described on page 56. The effects of these changes were so slight that they need not be discussed at this point; we shall refer to them again in connection with a later series of experiments.

We may summarize the more important phenomena of the objective records as follows:

I. There is a decided fluctuation in *errors* and a fluctuation, somewhat less noticeable, in the *rate* of tapping in each experiment. These fluctuations are such as to give 'groups' of errors in any given experiment. These groups reach their maximum at variable periods, but, in general, occur about two or three minutes apart.

II. The errors when arranged in two classes with reference to the more or less mechanical character they exhibit, follow this grouping in a very peculiar way; to wit, those that are considered more mechanical in their nature, class II, appear in groups that almost alternate with the first class or 'psychic errors,' and always appear first. Groups two and three of the errors seem to show a quite definite relation in this respect, group two being predominantly of the second type of errors and group three being principally made up of the first class of errors.[1]

III. Group two of the errors as preliminary to group three apparently appears at the point where, if emotional excitement be great, or the subject be in a subnormal physical condition, the danger of breaking down is greatest. Four subjects failed to pass this point in two or more tests apiece.

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[1] This relationship between the error classes offers a possible explanation of Cattell's observation referred to above, p. 20.

IV. The greatest number of errors in the less complex series fall in what we have called the mechanical class. Apparently the more superficial coördinations are concerned in the early practice effects in simple tests and in short series, and are more easily interrupted under the stimulus of the light fatigue phenomena originating in deep-lying sources during the early part of a test.

V. Increase in errors and increase in rate are not always jointly appearing phenomena; on the contrary, there is a much greater possibility that with a steady increase in rate there will be a fairly constant production of errors of both types.

VI. Individual variations are noticeably consistent with the general disposition and the particular physical condition of the subjects.

e. Introspective Accounts.

The summary of facts as indicated by the objective records is very much clarified by the introspective reports that accompany the records of each day's work. These records of the introspections have been used throughout the previous discussion to explain individual anomalies in the records. It seemed better, in order to indicate the actual relation of the introspections to the facts as they appear from a purely objective standpoint and to emphasize the importance of the introspections, to put these records in a more systematic form than any simple reference to them in the previous discussion would allow.

Imagery.—In recalling as the tapping proceeds, those who use visual imagery report that they simply "wait for the image of the next digit." When it doesn't come, they are compelled to recall the one just tapped or the preceding group in order to start the new one. Curiously enough when the image is lost, there doesn't seem to be any immediate group of sensations that may be called on as is distinctly done in the case of motor imagery; but there is the feeling of waiting for the image. Another method pursued is to start the tapping process in the hope that before too many taps have been made the digit will appear. Here the tapping clearly aids in the control over the imagery or the activities that condition this imagery.

Imagery alone does not prove sufficient to maintain a series in perfect order. The subjects report at different times the use of numerous and varied artificial means in continuing the process. These are used more frequently at points where the transition from one cycle to another is made. In actual 'breakdowns' resulting in errors, these artificial aids are conspicuous by their absence. Conscious control reduced to the minimum has literally failed in these instances to keep the requisite material on hand and the subject is left hopelessly lost and helpless regarding the next move. The field of consciousness at such moments is limited to the consciousness of being lost or of the last digit tapped. Upon the richness and closeness of relation to the previous process depends the possibility of proceeding without serious delay. The breaks caused by the material itself are, on the other hand, full of imagery and sensation processes, and it is only a question of proper correlation before the next step in the work appears.

Attention and Imagery Shifts.—In this connection two or three quotations may be made from the actual reports of the subjects. Subject 'Dy.' had, as we have mentioned, a distinct rhythm that was 'already in her finger' and she correlated this in the control of the series with the click of the lever and the heart beat. In this subject, we have therefore a definite method of control for the introspections. She reports that frequent errors were due to 'going off,' a light semi-hypnotic state apparently, and when consciousness returned she was lost. This return was accompanied in the earlier series by a distinct break in the rhythm, but later it frequently occurred without this rhythm check. In these later breaks the 'mind became a blank,' there was 'totally dispersed attention,' 'nothing would come.' When consciousness did return it frequently found that the finger tapping process was still going on, but the subject usually had little confidence in the correctness of the results. Subject 'A.' reports two types of consciousness connected with the errors, (*a*) simple mind wandering and (*b*) cases where the calculations went wrong or the tapping wasn't kept track of until the correct number of taps had been made. Sometimes subject 'A.,' who used visual imagery, could not get the digit to move in from the peripheral imagery field to the clearer center; it remained vague, blurred, and uncertain. In tests where changes were introduced all subjects report distinct feelings of relief, though frequently this did not mean that the new process went on any better than the preceding one. This shift was occasioned from without. But all report similar voluntary and involuntary shifts of bodily position and attention that were distinctly conscious and produced exactly such relieved situations. Unconscious changes of a similar nature could be observed, as when changes in the amplitude of tapping appeared on the record that found no introspective basis.[1]

The determination of changes in the visual field is far more difficult and uncertain. In some cases it is impossible, since the field is usually shifted continually, *e. g.*, as in following a number form. An introspection condensed from three observations by one subject follows. It is a partial attempt to describe the very vivid experience of this subject, but no confident correlation is or can be offered with the objective records or with the errors of these particular tests.

The principal point noted was the fluctuation in the visual field of attention. This was not the field of regard since the eyes were shut, but during the progress of the experiment the apparatus in front of the observer was more or less held in the focus of consciousness by means of visual images. In this whole field there was also a consciousness of the retinal blurs and after-images that never wholly leave the eyes of the subject even in the densest darkness. Both of these fields would vary in size and content, during the course of the experiment, somewhat as follows. The attention presumably wandered and this field would gradually narrow till it became a mere dark or light spot in the idioretinal field, then it would suddenly enlarge to include the most distant part of the apparatus or the peripheral portions of the field.

Accompanying this narrowing of the visual portion of the field there went a feeling analogous to that which precedes a fainting spell. Or it is similar to those conditions that appear when one has over-exerted one's self and a vague, immobile condition of consciousness is approached. There is also a feeling that this is very near the sleep stage of attention; that if something, one doesn't exactly know what in the beginning, would only give way, sleep of the most delicious kind would come. To offset this pleasant situation and, as nearly as the observer can tell, exactly within the state we are describing, there is another factor that prevents the consummation of sleep. This latter is a tense condition, somewhere outside the field immediately in the focus of attention. It is very difficult of location. It may be merely the marginal awareness of the digits as they are said in the course of the tapping, or the actual awareness of the narrowing field (though this last is certainly very indefinite since it is rarely detected until the condition described has passed away). It may be any one of the groups of 'strain' sensations that can at any moment be brought into the general consciousness. This tenseness holds on, however, and is the one thing that seems to keep consciousness from dropping away entirely.

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[1] We hope to show in certain control experiments that will follow that it is just such bodily changes and attention shifts that in a large number of instances prevent the subject from breaking down utterly under the complete unity of the process. 'Monotony' is perhaps the more common term for this situation, when described in terms of 'interest.'

During such a moment of inactivity, the difficulty of continuing the tapping seems very great. Only the digit that is being tapped at the moment is known. The subject feels at the mercy of the associative process and if it does not turn up the right digit at the close of the tapping nothing can be done about it. So small does consciousness sometimes become, that even the whole digit being tapped is not known. There is present merely a felt condition that something is being tapped, that there is a possibility of stopping when the time comes, or of the other digit coming in at the right moment and stopping the present process.

The 'strain' groups that have been noticed are: tenseness in the muscles of the ears; a group of 'strains' at the back of the neck, aggravated by the position of the subject; some that come from the muscles of the legs and feet where there are any places of complete stillness; from the trunk muscles; from around the eyes, the forehead, and from the back portions of the scalp as well. Not all of these appeared in the same narrowing period, nor was it noticed any time that more than one appeared. The point of origin varied with the position in which the subject was sitting, or the position of the head; the present vigorous condition of any portion of the body determining, apparently, in a very noticeable degree, the place that should first produce the 'sensations' thus noted.

In passing from this state practically the opposite set of conditions obtain. The field enlarges; the strain sensations drop out of notice; the apparatus in front is again visualized or the whole idio-retinal field returns; the digit being tapped, its position on the series, some notion of the place in the whole group that is being tapped, and of the time that the subject has been working, return to the observer. In such instances it frequently appears that some irrelevant thought has led the subject away. Once started the whole process seems to disappear and leave consciousness, as described.

Such introspections find corroboration in the experience of other subjects. Subject 'Wl.' found that the narrowing of the field occurred several times in the course of a simple automatic series and less often in the more complex series. Subject 'Pn.' reports a very interesting condition that finds its corroboration in the error 'grouping.' This is a temporary vagueness of imagery and it extends over two and three successive digits. He finds that an error will appear, due to some break in the coördinations, and before the thought of this conscious shift can be removed several other errors have appeared.

Sensations.—The introspective accounts are rich in material, but what can be given at this time must bear on 'tension' and 'fatigue sensations' alone.[1]

Source of fatigue.—After the first trials with the movement, the almost universal practice of the subjects is to cease reporting any fatigue of the lip, the one part of the motor mechanism that was in constant motion for the recording process. Cases in which fatigue was reported were found to be due to the position of the apparatus, to the reduction of the cutaneous sensibility by too much pressure, or by the rapid movement of the high speed tests. Subjects 'A.' and 'Ch.' both reported the special place of fatigue in the region of the eyes, forehead, temples, and scalp. Subject 'Ch.' also reported noticeable strains from the diaphragm muscles. All the subjects reported the fatiguing sensations of the continuous posture. The back of the neck was a frequent spot for the location of these sensations, probably due to the fact that the subjects felt that they must retain the same position with respect to the apparatus. Subject 'Rn.' found that the sensations appeared in any region of the body and that they seemed to have no local preferences; such was also the case with subject 'Ym.' 'Sd.' reported a desire to stretch and a general tendency to move at the close of the experiment. 'A.' reported greater fatigue in the lip than was reported by the other subjects. The major portion of this sensation was, however, a cutaneous one occasioned by the pressure of the apparatus against the lip. The separation of the two sensations, cutaneous and muscular, came out very distinctly for this subject in another experiment made to determine the relation of finger tapping to lip tapping. Here the obvious control in the first stages of the test was the cutaneous factor. Soon it became impossible to carry on the process with any degree of certainty and attention was found to move to a control that had apparently not been used up to this time as a separate conscious control, i. e., the kinæsthetic factors. These were used for the remainder of the test. This experience was reported by others in the lip movement and when questioned, none of the subjects has been able to locate in the actual muscles used the fatigue that came temporarily. This 'fatigue', or numbness was always located in the cutaneous region pressed upon by the apparatus and not in the deep-lying tissues.

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[1] In this connection, is appended an introspection made by the writer in one of his first tests with the apparatus when there had been practically no habituation for the apparatus.

"During the experiment several groups of muscles came into prominence that had not been in action at the beginning of the test and were not used in the test when only an average attention was used. Noticeable were the tension of the forearm as it rested on the table, the tension in the forehead, the increased tension of the back of the neck, and extending up to the back part of the head farther than usual, an increase of tension in the lips, the eyes, and about the head generally. The lips did not appear in the least tired, i. e., report the fatigue sensations, which were in every case apparently reported as from the points of origin mentioned above. The lip movements did in some instances show a tendency to slow up. The attention wandered in several instances, and the errors seemed to bunch themselves more than was noticeable in the former tests. This variation in attention was corrected by turning the attention not to the next number to be tapped but to the lip movement, then to the digit. It seemed to be necessary to get control of the lip movement in a firmer way before anything could be done with the mental process."

Tendency to Sleep.—Four of the subjects reported an overwhelming desire to go to sleep during the course of the experiment. This sleepiness had to be fought off continually after the first control over the material had been obtained. Apparently it constitutes a hypnoidal phenomenon similar to the experiences reported above wherein the field of consciousness was reduced noticeably during the course of the test. In such experiments, we have a single process offering little in the way of variety, yet requiring a steady strain of attention during its course. There are no extraneous conditions except those that are as continuous as the process itself, the stillness of the room, the whirr of the motor; a typical hypnotic situation. The result is that the sleepiness induced has little or no imagery in it. When sleep appears, the dream imagery is frequently the imagery that has been pushed out at the beginning of the experiment. In one instance this amounted to a dream of some proportions. The subject found herself just ready to embark on a steamer for Europe. When almost ready to step aboard the steamer, a question was asked her in the dream and at the moment of the decision regarding it, she awoke to the tapping once more. Typically opposite results were obtained from three other subjects.

Several times just before the experiments, each of the three subjects, as they recalled the course of the previous tests, remarked that they felt as if they would go to sleep this time. But in no case was any desire to sleep manifested during the experiments, though mind wandering and mental lapses were reported frequently. These subjects actually reported in all these cases that they were much wider awake at the end of the experiment than at its beginning; sometimes highly excited at the close and eager to go on or do something else at once.

In connection with these varying reports, we may mention a characteristic change in the breathing curve. Those subjects who reported the tendency to sleep exhibit, in experiments where a breathing curve was taken, a very slight but definite slowing of the rate of breathing. In subject 'Sd.' this amounted several times to a decrease of two respirations per minute during the course of a twenty-minute experiment. Subject 'Dy.' who manifested no tendency to sleep during the experiment, presented considerable irregularity in the breathing curve. Only once did she show any tendency to breathe more slowly during the course of the test. This one was the test in which there was no danger of exhaustion and a decided decrease in the rate of tapping. For one or two of the subjects, the breathing curve shows at fairly regular intervals a deep inspiration. Only some thirty of these breathing curves were taken, so the results cannot be more than provisionally related to other characteristics of the work curve.

SUMMARY.

It is hoped that enough of the large amount of material gathered in the course of the experiments has been presented to indicate the value of introspection, uncertain as it is in such continuous series. Among the details of the introspections presented, special mention may be made of the following:

I. 'Sensations of fatigue' arise suddenly, that is, they appear in consciousness and take the center of the field at times, where they function as ordinary sensations. At other times exactly the same situation produces, not an attention process centered on the 'fatigue sensations,' but one focused on some other part of the process, e. g., something in the field of vision, in the other imagery of the process, a movement that appears afterward in consciousness, or on any process whatever except the immediate image that would have continued the work in hand.

In these situations, we would suggest that the sensory processes have been instituted as before, but the coördinations are established, and instead of producing a demand for conscious control, the stimuli have resulted merely in a modification of the process on the basis of the immediate habits used in the present situation. The length of time that these sensations function as conscious processes depends probably on the working habits of the individual, the relative difficulty of the present situation, and upon the length of time that the subject expects to continue the task.

II. When the introspections are made at the close of a fifteen or twenty minute experiment, errors are reported as due to breaks in the rhythm of the process (*a*) from unknown causes, (*b*) from sources within the imagery or mental work of calculating, (*c*) interference of outside stimuli, (*d*) appearance of ideational processes from unknown sources, and (*e*) *sensations which when localized interfere definitely and when only vaguely present tend to slight degrees of confusion.*

III. *Introspection* reports the greatest difficulty as arising from two sources, first the monotony of the process, i. e., the loss of the excitment and the tendency to lose the stimulating 'strains.' In the second place is to be mentioned the difficulty of maintaining the tension (attention ?) out of which the particular mental process is issuing. This tension, if lost, means that the conscious process will also be lost. *The maintenance of this situation seldom involves a steady, specific strain.*

Discussion. In the progress of a particular mental activity, there appears from the above to be for each individual a core of closely related sensations and images, based partly on the ideational types that are commonly recognized as motor, verbal, auditory, visual, etc. As a test of this hypothesis let us try to describe what is to be expected in any single case of incipient fatigue. If our particular individual selected for the test be of the hand-motor sort, then we may expect such things as writer's cramp, the feeling of limpness in the hand and the wrist, or the rise of that more indescribable thing called an indisposition to work, lassitude. The proper coördinations that have been furnished the 'vis a tergo' are receiving less and less of the stimulating sensations that constitute the 'tendency' to mental activity.

In these experiments, the control of the process does not seem to reside in the lip movement, but rather in the tensions that constitute the basis of the motor process. This control is of a sub-conscious character and not in the focus of consciousness. When the tensions do come into the focus, it is by means of a distinctly new set of sensations, 'fatigue sensations.' The entrance of these sensations is the 'cue' in every instance noticed for a change of attention either to the point of origin of the sensations themselves, or to some more interesting topic of thought.

The recovery from wandering attention or from any difficulty of recording is not always first through a mental image, but frequently through some reference to the particular muscle group guiding the record. Any deep-seated disturbance of the sensations ('stimulating factors') arising from the particular points under tension indicates that the focus of attention has varied. Tension rather than movement is the immediate controlling factor. Though movement may constitute that portion of the process that is determining the locus of the tension, the tensions themselves may frequently be elsewhere than at the places of overt movement.

The location of the controlling sensations is not usually noted in any disturbing sense until the sensations that arise from that portion of the process are actual 'fatigue sensations.' The response to this is a relaxation of the point of tension and a tendency to movement in the parts affected. In the muscles actually moving in such experiments as these the fatigue sensations are practically *nil*, and are not the ones that make us say we are 'tired.' These parts are best irrigated and are thus kept in condition for their work. The other parts are in less satisfactory condition. This seems to point to the theory that fatigue is due to the excitation of special nerves by the accumulation of one or more of the three fatigue substances beyond a normal amount. This accumulation appears first in the tense non-moving muscle. The accumulation finally takes place in the muscles that are working but always a considerable time after the fatigue sensations elsewhere have superseded the sensations of tension and strain. There was no experiment at this point to test the possibility of transference of the control process, but if this description be true, it seems to indicate the possibility of a way out of the attention-wandering experience entirely. If the organization of the muscle controls be closely enough connected and arranged in serial order through habit, etc., there will be no need of any attention-wandering at all. For example, on the basis of habit the effort might be transferred from chest muscles to abdominal muscles or the tension be even moved to muscles of the extremities in the course of a single piece of mental work. This transition can be made on the basis of habits previously established, and while the mental process is going on, the transference of control from one fatigued set of muscles to another could take place without disturbing the continuity of the specific mental process.

f. Control Experiments.

1. Using a rapid rate of tapping.—Certain control tests were made to test the validity of this view of mental fatigue. We have at various times mentioned a group of experiments where the rate and conditions were made comparable to others' tests of this general nature. This high rate is, of course, impossible of direct application to the more difficult calculation instituted in some of the series. Instead of asking for a high rate a combination was sought that gave the most rapid rate possible and still kept the errors within limits less than one error per digit of the cycle. This enabled the observer to feel that the work was going well and that special confusion on account of the speed did not enter.

TABLE VI. Errors.

SUBJECTS	Ym.				Ch.			Rn.		A.
SERIES	a	b	c	d	b	c	d	a	b	a
1st min.	4	4	2	3	1	0	0	3	8	5
2d "	0	1	1	0	3	0	4	3	1	5
3d "	4	5	2	7	3	1	1	4	5	3
4th "	5	4	1	2	1	1	2	16	8	1
5th "	1	1	2	4	2	1	4	8	18	0
6th "	3	3	1	3	1	3	3	5	14	4
7th "	4	4	2	2	4	0	3	8	7	2
8th "	?	2	5	4	6	2	1	5	7	5
9th "	4	2	1	5	3	3	5	6?	?	5
10th "	2	2	4	2	6	1	3	7?		1
11th "	5	4	5	4	4	2	0	15		3
12th "	6	2	2	3	10	0	5	8		4
13th "	7	3	1	2	1	1	3	11		7
14th "	4	6	5	9	7	1	4	13		1
15th "	4	5	6	3	5	1	5			3
16th "	9	3	6	5	3		4			7
17th "	10	2	1	0			1			
18th "	2	2	3	5			3			
19th "	2			5			5			
20th "				6			3			

Table VI. Selected from results of 'rapid series' tests. The rates, with the exception of speed, are so nearly comparable to the preceding tables that they are not given. 'Series' in the table indicates the order of tests as given on successive days.

Table VI shows the results of the 'rapid rate' experiments. The initial group of errors is lengthened and group two pushed up on this account to a position near the third group which occupies its usual position. Otherwise, the errors appear in their ordinary manner, slightly increased in amount in subjects 'Rn.' and 'A.' 'A.' is particularly noticeable in this regard, since his records in the earlier tests are quite free from errors. The very great proportional increase in the rate indicates the main reason for this change in the errors. We stated that the distribution of errors within any single experiment does not depend in any appreciable degree upon the rate used in the experiment. These tests suggest that the absolute rate of an experiment determines in large measure the total number of errors that the whole experiment will contain. This tendency is modified, naturally, by the proportionate difficulty of any cycle of digits and its accompanying calculations. We may repeat this as follows: *The absolute number of errors in an experiment depends largely on the rate and difficulty of the experiment, while the distribution of errors bears slight but positive relation to the changes in rate that occur within a given experiment.*

Introspectively, such attempts at speeding indicate the futility of attempting to get at an absolute rate of the mental process through such motor processes as these; i. e., isolated motor accompaniments that are correlated with the mental processes simply for the purposes of such recording. The motor process of recording can in no wise equal the speed of the complex motor adjustments (fusions) that are in charge of the imagery in adult experience. The mental processes wait throughout on the recording apparatus and the approach to them is not through the speed of isolated movements in any one portion of the body, but through the coördinated activities of the entire organism with reference to this momentarily isolated portion. Thus shifts in the recording movements affect the coördination not merely by relieving the particular part that happens to be active, but also by producing a rearrangement of strains in the total coördination.

Effect when finger is used to tap the digits.—The change from the lip-tapping to the finger-tapping, where the subject had used the former through the greater part of the year's experiments, shows no extreme variation on this or other points. The selection of the lip as the ordinary method for the experiments enabled us to make this comparison between the establishment of a new coördination and one that was highly automatic. Possible differences appear. The rate is more rapid. Certain rates already exist in the hand mechanism that cannot be modified on the spur of the moment and must be used to carry out the mental series. This rate represents quite fairly a regular speed for the coördinations of the hand. Differences between the right and left hand indicate a modification of this proposition that is quite interesting. *When the right hand, lip, and left hand, were used in alternation to record the cycles within* *a single continuous mental series, the left hand showed the highest rate throughout the earlier tests.* Only subjects that were right-handed were used on this experiment. But the result seems to indicate that we are dealing with a relationship between two separable conditions. Certain rates are worked out in the hand mechanism, the right hand in these cases is primarily used to establish this rate and is thereby more closely and definitely related to its appropriate coördinations and 'strains' in the deeper proprio-ceptive mechanism. The left hand follows by the transference of practice and irregular trials of its own, attaining a speed approximately equal to that of the right, but lacking the intimate and more thoroughly established connection with the deeper coördinations. Two things follow from this situation when a new mental process demands expression. The left hand starts off at the rate 'artificially' imposed upon it by the right hand from previous practice. As opposed to this the right hand because of its closer relationship to deep-lying coördinations indicates at once in its slower rate the difficulties in the new mental process. In the second place consciousness reports this condition of the left hand in the awkwardness of the tapping and the more rapid rise of 'fatigue sensations' and fatigue itself.

Highly complicated series.—In a group of twelve experiments especially arranged and ten others arranged for another purpose, the calculations and complications of the mental process were raised to the limit of the relation between the retentive powers of the subjects and their ability to calculate during the recording process. The rates showed nothing beyond a slightly higher practice change and in one subject, 'Ch.', where the work was quite difficult, much higher fatigue effects. The errors follow their characteristic changes with two exceptions Group one tends to continue longer than usual, a point noticed in the 'rapid' series above, and there is a slight masking of the later groups in the records of 'Ch.' because of emotional states brought on by the confusion attending the difficulty of the series.[1]

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[1] One striking instance out of the many, for the necessity of careful checking on these introspections may be mentioned. A cycle of ten digits, 3, 2, 2,

New directions during the test.—To test the continuous complications of a series, a group of experiments was carried out in which the subject received the directions at variable periods through the course of the experiment. The subject would start on a series of digits memorized beforehand and at intervals selected by the experimenter would change the calculations on the memorized series of digits. Ten and twelve different directions of the nature of the calculations described in the experiments of group II were given in a single test of fifteen minutes in length. Care was taken in the series to make some of them as homogeneous as possible. Others were started by an easy direction or two and ended by the same direction, or ones similar in their degree of difficulty, in order to test the possibility of general fatigue and the effect of the intervening difficult calculation on the automatic processes.

Such tests obscure the results as found in the other experiments by increasing the total of the errors throughout the test to a point that practically equals the errors of the first part of all the other tests. The tendency to disturbance in coördinations and 'strain' groups is far less noticeable. In fact, it is practically unnoticed because of the demand on the attention and the interest attendant upon the frequent introduction of a new form of calculation. This undoubtedly takes the place of the shifts and error groups that are familiar in the continuous homogeneous series of the other experiments.

Alternate tapping.—The second question, raised in connection with fatigue sensations and the 'strain' sensations, was regarding the possibility of reducing the error fluctuation by

4, 3, 2, 4, 4, 2, 3, was given and memorized. The tapping was begun and continued for thirty seconds when the subject stopped and reported as follows: her mind had become a blank and no imagery could be found (after the final correct taps about ten seconds were spent in an effort to recover, according to the record). In repeating the cycle as the double fours were noticed, she thought that these would be difficult to recall in their proper positions and while thinking of this danger finished the series. Here *according to the introspection* the mind became a blank and no more images appeared. The record shows that the cycle was tapped once correctly and up to the second four of the pair the second time with no errors. The thought of the specific difficulty had erased all memory of the process that had continued during the persistence of the idea.

changing the muscle groups used in the recording process itself. Schuyler and Swift, in their paper, indicate the decidedly positive character of typewriting as far as the reduction of localized fatigue sensations is concerned. This observation is one that is made by many users of the typewriter. The localization of these sensations is much more difficult than when pen or pencil is used. An analogous observation is presented in the experiments of Sherrington where the muscle response is elicited for a considerable length of time after most observers have reported fatigue, simply by moving the place of stimulation. Exactly the same sort of issue regarding so-called intellectual or mental fatigue has been experimentally brought out by the work of Lombard and Hall with the ergograph. As a theoretical result, the fluctuations in errors and mind wandering may perhaps both be largely eliminated by the arrangement of a series of tests that alternate the processes used in tapping the records. Thus the habitual working rhythm of the various subjects could be eliminated so far as concerns an inferior quality of work appearing at certain periods. A tentative test of this was made on several of the subjects. It involves, however, for complete control a definite knowledge of the physiological rhythms of the individual and also of the habitual methods of working, so that nothing more than a preliminary illustration of what is meant can be offered at present.

The preliminary apparatus for this test is as follows: the lip arrangement for recording was used, and in addition the tambour attachment that was used in the finger tapping was arranged for both the right and the left hand. These three tambours were attached to the same recording tambour and the record run as a continuous one on the drum. (This control test is the one mentioned above in the preliminary statements regarding apparatus.) Highly complex memory series were used in these tentative tests. Hence the entire significance of the change in recording is not shown. Notwithstanding all the difficulties indicated, the records that were taken show a decided disturbance of the usual rhythm of fluctuations and show plainly that we are not dealing with an isolated mental process when we speak of the quality of work and of fluctuations in intellectual work. (It is in these experiments that the records of the left hand show, in the two or three earlier tests, a rate that is regularly higher than that of the right hand.) The introspective account of the changed method is a decided one. All five of the subjects who tried it were clear in the conviction that had they been using this method throughout they could have eliminated many errors and have been much fresher and more interested in the course of the experiment. Subject 'Ch.' still reported slight fatigue about the eyes. The difficulty of the experiments in her case and the necessity of using the number form in order to remember the series materially modify the conditions of her record. The method of recording is not calculated to modify this condition to any extent. We feel certain, notwithstanding, that no one would think of looking for error groups or regular fluctuations in this series of records.

SUMMARY.

The actual mental condition of an individual at any time is a function not chiefly of the particular set of muscles in operation, but of the ability of the person to maintain the more fundamental strains of the entire body. Of course, whenever the method of recording be such that the muscles recording the work done must enter into the qualitative changes that appear on the record, then the results will be modified materially by such conditions; for example, if the muscle gets excessively tired, this abnormal condition of the muscle will affect the records of the mental work being done. *These error groups are, however, in our view an argument against the possibility of considering mental work as anything apart from specific, coördinated muscular response.* These coördinations are, moreover, not to be confused with particular muscular responses. The mere recording should not materially affect the real results of the 'mental' work. It is to be noted that this must be said with a reservation respecting secondary effects that the working of any group of muscular responses may have on the 'strain' conditions that already exist.[1] The effect that the transition of the actual and specific activity from one recording muscle group to another will have on the physiological conditions that are maintaining the heightened cerebral activity, should also be noted.

It is not denied that the particular *deep-lying* group used in recording is affected. Both as a working muscle and as a part of the neuro-muscular process, it will exhibit all the phenomena of mechanical stimulation of muscle and will also mediate sensations that are real fatigue sensations, affecting in both ways the smoothness of the cerebral activity.

Here we are dealing with a more general phenomenon, one that is not conditioned merely by the possibility of the circulation diffusing fatigue substances, generated in the working parts, to the more remote parts of the muscular system. Diffusion of fatigue substances cannot be made to explain the fluctuating phenomena of the error curve nor the introspective report of those subjects who report definite recoveries in their ability to do the work in hand. These recoveries are independent of any possibility of changes in the circulation as such and of the distribution of a chemical substance throughout the body. In other words, these sensations arise suddenly while the production of fatiguing substances must be considered as going on at a more regular rate, or, at least, diffusing more regularly.

A shifting of the 'strains,' that in themselves are productive of fatigue substances, in cases of high mental tension will account for the automatic shifting of the specific seat of the strains and the production of a new center for the 'vis a tergo' sensations. In the majority of cases this transition is not accomplished unconsciously and we have the appearance of the fatigue sensations and the habitual attitude of the majority of workers with respect to these sensations; the loss of interest, the shifting of attention, the feeling that one must do something else since this new mental condition is considered indicative of a need for rest. As a matter of fact all that it does indicate ordinarily is that *a center of kinæsthetic activity is calling for readjustment.*

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[1] The numerous and interesting effects of different incentives upon the amount and nature of fatigue have not been made a subject of investigation here. They are, however, relevant to the main problem, i. e., the relation of sensations of fatigue and amount and character of work done. Wright's article (108) and Bücher's (17) indicate fruitful lines of inquiry on this particular point.

IV. GENERAL RESULTS AND CONCLUSIONS.

Before passing to points of general significance that the data and form of statement of this paper have suggested, reference will be made to the specific points that were offered as problems most insistently rising out of the previous investigations.

Physiological conditions.—The experimental work outlined here suggests what seems to be a very fruitful field in the arrangement of experiments in continuous activity. This is the effort to get conditions that shall approximate the conditions that obtain when the subject is working under the impetus of normal incentives. Naturally, such an ideal means the introduction into the experiment of somewhat of the complexity of the study table, and more of the atmosphere of the investigator into the attitude of the subject. It seems to the writer at least that on psychological grounds, an investigation ought to lead somewhere for the subject as well as for him who plans the test.

The plan of the experiments here carried out has tended to produce in the minds of those who have watched them as they have grown, the conviction that it is only by such 'at will' methods that the correlation of the physiological and psychological facts is going to be brought about. For example, *the work of this paper suggests still more strongly what lies in the background of most psychological investigation, that the rhythm of certain highly automatic processes modifies materially the character of a short and circumscribed experiment.* Had the writer developed more fully certain other material that seems to lie beneath the more obvious data that have been offered, he would argue specifically for the correlation of qualitative changes with vasomotor rhythms and similar physiological processes. For example, it seems quite clear in many cases, that the errors while grouping themselves in these larger 'muscle' groups also tend to scatter single errors along the course of the work in arrangements that suggest these more fundamental physiological rhythms.[1] At present such statements must be left for further investigation and more thorough corroboration.

Moreover, such problems on the road to solution will enable the psychologist actually to make an adequate analysis of the complexes he so trustingly terms kinæsthetic sensations, organic feelings, and more recently 'attitudes.'

Furthermore, such a method presents the possibility of a less forced attainment of the ordinary conditions that are set in the laboratory. Several of our subjects present the spectacle of attaining the maximum of attention and interest without the insistence of this through express directions from the experimenter. We are thus offered a chance of investigating the actual 'warming up' process and the varying time in which it is attained by the same subject on different occasions and by different subjects under approximately similar conditions.

Fatigue and practice.—The relation between fatigue and practice ought to receive further illumination from such methods as are advocated here. In the secondary emphasis that it has received in this paper the conviction that they are not related in precisely the close correlative fashion that certain investigators have thought them to be, has become more firmly fixed. Fatigue as shown by our work, by Thorndike, by Ebbinghaus and others, is, first, a very immediate function of the mental process, and, secondly, a reduction in the general capability to maintain the production of a maximum amount of work. We have stated above our own conclusion as the curves of our experiments seem to indicate them.

We may offer here as a result of this point of view, the fact that immediate fatiguability is frequently independent of practice power and practice gain (in this statement we do not go beyond the suggestions of Wimms or Schuyler); that the possibility of practice and the retention of practice does in a very real sense depend upon the general fatiguability of the organism. By fatiguability of the first sort we mean in this statement that which shows itself in definite fluctuations in quality and quantity of work during continuous work. This is not the fatigue which Kraepelin argues is related to the practice curve. General fatigue according to his definition and ours actually diminishes the power to work for long periods, and is correlated with practice.

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[1] The work of Bonser (13) certainly offers very striking corroboration from the physiological side of the presence of these longer two and three minute fluctuations in the mental work, the discussion of which constitutes a large part of the experimental portion of this paper. Space will not permit a discussion of the numerous articles on daily rhythm, individual working hours, etc. These have been quite carefully summarized in a paper by Marsh (60).

Without the 'fluctuating' type of fatigue in mental or muscular work the possibility of any extended effects of practice ever getting into the deeper co-ordinations or 'strain' groups of muscles is very slight. We meet an analogous argument in Sherrington's discussion of 'negative induction.' (See pp. 9–10.) In the properties of the synapse he finds resident the possibility for the large variety of sensory stimuli to which the organism is open. If 'stimulus and response' from a single center were not subject to a diminution in activity by means of that activity itself, there might be, for example, an eye of far greater penetration than at present exists, but such specialization would mean the absence of, or highly diminished power of, other sense factors. Thus the fluctuations in mental activity conserve the power generated by the specialized process by shifting that activity from time to time in such a manner that the effects of the activity are gradually diffused to a more far-reaching co-ordination than is in immediate use. This transference may, of course, take place in both the positive and negative planes of transference of practice. As a result of either transference we have the so-called habits of work generated, and in the majority of adult individuals the distinctly apparent reduction of early plasticity and capability to take on practice gains (i. e., negative transference).

We should therefore agree in the main with the statement made by Seashore that fatigue is not general but specific in its nature. The type of fatigue that has been most in evidence here is capable of definite localization and description and belongs to the realm of psychological investigation. We do not contend that the work curve is not dealing with real facts. It seems, however, that shifting the controlling mechanisms tends actually to increase the metabolisms throughout the neuro-musculature of the body. The generality of the fatigue symptoms that are dependent on decrease of protoplasmic structure in the tissues, is in some degree proportional to the susceptibility of the muscles to shifts in the coördinations, or to immediate fatigue. These facts are in their nature opposed to a general decreased power to work.

Quality of the work.—The number of these changes and error fluctuations, together with the rate of fluctuations, indicates the general habituation and endurance of the individual. This will be true on the same grounds that Kraepelin argues for the relation between the 'equilibrium pause'[1] (Gleichgewichtspause), and the 'most favorable pause' as an indication of the high or low fatiguability of the individual, i. e., in terms used here, the greater or less susceptibility to coördination shifts. The quality of the work ought therefore to constitute a very important criterion for the evaluation and actual location of the factors involved in an objective work curve.

Consciousness and fatigue.—The complex of psychic accompaniments of fatigue is so varied and so common an experience that the attempt to analyze it into simpler groups has always seemed hopeless. We are confronted with 'feelings of effort,' 'feelings of well-being,' 'exhaustion feelings,' 'strain feelings,' 'sensations of fatigue,' 'lassitude,' 'limp' sensations, 'laziness' and a score or more that indicate the bodily attitudes from time to time. Out of the individual parts of the complex, formed by it, and determined by their proportionate relations, the 'feelings of effort' seem to stand as the final psychical representative. Beneath and just within the consciousness of effort we usually locate the feelings of strain; in serial order, they appear after the whole complex has had its turn in consciousness. Outside of these, appearing later still, and usually viewed as disturbing factors, we find the 'sensations of fatigue' placed. These are popularly supposed to be correlated in a negative fashion with what we have included in the descriptive term 'feelings of well-being.' It is not our purpose to maintain on the basis of any cursory analysis of this complex that these are all to be separated into clearly differentiable sensations. As a matter of fact one or two of them seem to belong to categories similar to those of space and time perceptions.[1] Outside this debatable ground, however, there seems to be sufficient evidence to attempt the isolation and description of the fatigue sensations. In the analysis offered certain distinct types of objective fatigue also appear with some distinctness.

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[1] By 'equilibrium pause,' Kraepelin means, as was previously stated, that pause where the amount of work done after the pause in a unit of time is equal to the amount done just preceding the pause. To determine the locus of such a pause, certain factors, as the loss of 'incitation,' decrease in fatigue, the introduction of an Antrieb at the recommencement of the work, etc., must be taken into account, hence the use of the technical term 'weighting' in the descriptive phrase.

Types of fatigue.—1. The first group of shifts in coördinations is that one which gives us the varied imagery of our most rapid conscious processes and is undoubtedly conditioned by precisely the same physiological situations as is the phenomenon designated by Sherrington as 'negative induction.' The varying length of these changes (from the immeasurably short shifts and 'blockings' of the more finely adjusted musculature that report themselves in consciousness as wholes of pleasantness and unpleasantness, as confidence in the possibility of doing the thing in mind, etc., on to the grossest muscular contractions that finally cease to respond to the effort of will) can be explained by the gradual refinement of the sensory end-organs lying within the muscles and their gradual coördination through long periods of use and practice with the neural pathways and synapatic mechanisms.[2] There is no apparent reason why these changes may not go on indefinitely, following their own internal lead, as in reverie and mind-wandering. There is also no immediate psychological reason whereby to explain changes such as are usually found in interest and monotony and so on, beyond this 'fatigue' shift. As convenient modes of description, interest, monotony, etc., find their explanation in *the establishment of habits of thinking and of acting*, rather than in anything that is inherent in the objects themselves. The biological reason for shifts is well stated in the 'negative induction' theory of Sherrington.

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[1] See articles by R. Haynes in *Jour. for Philos. Psychol. and Sc. Meth.*, 1907, v. 4, pp. 601–606, on Attention, Fatigue and the Concept of Infinity.

[2] From this point of view, it seems almost impossible to place the actually controlling locus of the nervous 'blocks' in such evanescent functional units as the synapses. It is undoubtedly true that in them may be located the immediate cause of the shift to some other neurone system, associative or projective. The point, however, where it is first determined that this neurone pathway has acted to its capacity in this particular place and at this particular time, and the point where it is determined what neurone system shall follow, is not originally in the synapses but in the muscles themselves. It must not be forgotten that in so describing the situation we are aware that in the majority of instances

Consciously, we say that our attention could no longer be held by the object in question, (we even attempt to measure the length of time one thing can be focussed by the attention) and we describe the arrival of new objects of attention and of new thoughts as due partly to the loss of interest in the old, to the lost fascination that any particular object may have had for us. These descriptive phrases seem explained on the basis of this habitual factor that has ingrained itself in the muscles and nerves of our organism. This neuro-muscular mechanism has come to offer the shifts at certain times and unconsciously we accept them and look at the object from a 'new' standpoint. At other times we consciously accept them as the 'gospel' and say that we must do something else or think of something different, retire, rest, or take recreation, must do anything in fact but modify the situation by refusing to be dominated by one single set of responses and, if this set fail to work, call upon another set or adjustment that will. A coördinated habit of mind wandering is the result; incomplete concentration in any task becomes the unvarying habit.

2. The above described process is more or less automatic; our mind wanders and we find it out afterward; we are not conscious that this has taken place till we find the mental process engaged on something that reports itself as different from that

this coördination is not a conscious one, but has dropped into the realm of automatic activities. In others instead of being manipulated directly by a conscious process the direction of change has become an habitual mode of response. In still others, a possible majority of the instances, the change is manipulated from the standpoint of the exteroceptive system, sense-organ adaptation and fatigue.

which started us off. But there is a more definitely conscious shift than this *post hoc* sort of consciousness. It is one that offers confirmation to the above statements. Within an habitual mental and muscle group tasks can be arranged to perform which will require the expenditure of more energy than is ordinarily used in the automatic process of shifting the coördinations. In other words, our habits of thinking are not usually 'set' for the longer periods of time. As Professor James (46) puts it, we are ordinarily working far below the level of our ability so far as the consumption of stored energy is concerned. This is, nevertheless, the condition that has led to our habits of shifting and forms the logical basis for the constant shift of attention that we find generally in the animal kingdom.[1]

In human life we can and do get conditions that demand the expenditure of continuous streams of energy. As a result, we have a more or less homogeneous piece of mental work continuing over long periods of time, that cannot be explained on the basis of a single adjustment of a single set of muscles or a single muscle. A second and higher habit is formed, the coördination of kinæsthetic shifts. If it were physiologically possible and biologically and methodologically valuable, as individuals we should much prefer to have the control carried on by a single group of muscles till the mental work had been completed. At this point, however, the chemist shows us substances produced in the working muscle, strain or active contraction alike, that are inimical to its continuous activity. The biologist tells us that the specialist, as a mere individual, must fail in the great life functions. The scientists generally demand not only habits but the power to break those habits. Each and all abjure over-specialization.

On the conscious side, if we push the muscle beyond a certain stage of activity, we are awakened to the fact by the appearance of a new phenomenon, 'fatigue sensations.' These are from this general viewpoint the correlates, on the psychological side of the process, of the 'negative induction' and automatic shiftings that we have described above. Two things are effected by the katabolism of the muscle under tension, the first is the functional shift from one synapse to another, the second is conscious and it constitutes the warning of the changes in the situation. Here is the point at which the difficulties of our analysis become apparent.

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[1] Its biological variations are interesting. The cat sitting patiently at a hole awaiting the appearance of its mouse, the sudden stillness of the wild animal at the appearance of a stranger in its native haunts, indicate instinctive modifications of this internal organization.

Fatigue sensations are of a certain definite kind but they are so intermingled with other sensations, of effort, of pain, discomfort and the like, that they are scarcely isolable in ordinary situations. If we take the experiments on the ergograph, we shall find three distinct groups of conscious factors present. (1) The 'feelings of effort,' (2) the pains of torn tissues and muscle fibers, and cutaneous pressure and pain, etc., and thirdly, as the experiment continues, we can notice a group of sensations that are neither the one nor the other. These sensations may be described as if the muscle, if taken from the body and compared with one in a fresh condition, would feel softer, less resilient, less elastic and more shapeless. With this feeling there is, of course, associated the change in position as reported by the muscles themselves and by the tendons and joints. These sensations, so introspection further reports, would be decidedly pleasantly toned if it were not for the intrusion of the disagreeably toned pains and the drawing off of attention by the insistence of the feelings of effort.

Waller suggests that all three of these sensation groups are mediated by the same end-organs within the muscle. It seems more nearly correct to say that 'fatigue sensations' are just as often present with the other two as appearing at any definite stage between or after the pains and feelings of effort, as his theory would indicate. Furthermore, they seem to be the opposites, not of pain and disagreeably toned sensations, but of feelings of well-being and the condition of perfect possibility of action on the part of the muscles involved. The feeling of effort and the actual expenditure of effort as Mosso has shown by the ponometer and Trêves by his forearm ergograph are quite definitely correlated and they arise in a graded series from the beginning of the experiment to its close. In other words, they form a conscious criterion of the general bodily energy in themselves.

Exhaustion.—3. The final stage of the feelings of effort, when they do not shade off into 'strains,' etc., is exhaustion, not fatigue or reduced capacity for work. Exhaustion is a phenomenon of complete loss of control and complete expenditure of effort where the fatigue sensations are almost entirely lacking throughout the experience. The above described conscious reports of fatigue are, thus, evidences of the normal demand on the part of the organism and the muscles in particular for a shift in the working mechanism.

Definition.—Fatigue presents itself in innumerable ways, but in general it may be said to express the situation underlying all regular and irregular rhythmic activity of the organism wherein the central point of that activity tends to shift toward other centers. Fatigue is conditioned by the functional interplay or 'excitement' that exists between muscular and nervous tissue. As a conscious phenomenon the locus of fatigue is primarily an activity of special end-organs lying within muscular tissue, referable apparently to 'deep sensibility' and the proprioceptive system of 'arcs.' Under this general characterization fall what we have termed automatic changes, rhythmic fatigue, conscious fatigue, sense organ fatigue, exhaustion, and fatigue, due to lowered metabolisms in the tissues. This statement attempts to express only the positive phase of the varied phenomena described.

Motor and neural relations of fatigue.—The phenomena as far as the experiments here reported can tell us are quite similar to the fluctuations that are found in the attention process, and in other mental processes where the control is exercised immediately by the 'strains' that exist in the paired muscles and by the organic sensations. These will be, if the unit group happens to be small enough, practically the same as though an external control were used. That is, the sensations will arise and will be located with all the definiteness that obtains when we say that our eyes or ears are tired from seeing or listening. When we use such motor controls as the hand or the fingers or lips, the one difference that is found seems to be that the sensations that arise do not appear first in the muscles immediately concerned in the activity, *but in those parts that are more or less quiescent as far as actual noticeable movement is concerned, and are essential in the whole situation from the standpoint of 'strain.'* For example, in those muscles that control the eye in the visual reaction, or exercise control over the ear, we have the strain condition predominating over the actual work of the moving muscle. In such cases we have the fatigue sensations arising nearer the seat of the activity itself.

In movements that are run to their highest limit of speed as in the tapping tests, and all recording, we have similar results. No motor activity of a single more or less isolated group of muscles can approximate in rapidity the activity of the train of imagery.[1] When we come to piano playing, the use of the typewriter, and the larger groups of muscular activity, we are approximating the situation that we believe to obtain in a less (externally) striking way in the coördinations of shifting strains and tensions that control the mental activity of the individual. The speech organs furnish undoubtedly the best organized of all these definite, externally observable movements. *In all such systems of coördination, sensations of fatigue arise not in the muscle that is working under regular periods of contraction that are timed to suit the habitual rate, but in those portions of the muscle or muscles located at any place in the body that are under tension in guiding the activity of the muscles that are working, i. e., in the rigid muscles.*

In our historical review, we have seen that the present tendency is to refer the control of muscular activity to peripheral parts and to assign a more secondary function to the centers. The richness of the sensory end-organs in the muscles and the immense number of pathways leading from the conscious centers to them argues strongly for the reference of much more of our conscious life to the sensory stimuli arising from these sources. It is still true that the necessity for readjustment appears more frequently, at least in the adult, from without and through the ordinary avenues of the five senses. However, when this material seeks to become a part of our activities, and seeks to find its place among the coördinations already present, the fundamental importance of these sensory controls arising within the muscles begins to be apparent.

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[1] For a detailed discussion of the points involved see Burnett's and Woodworth's articles in the Garman Commemorative volume and an article by Miss Downey (27).

As indicated by the previous discussion, we would argue that the so-called mental fatigue has an origin that is also peripheral or muscular. In the first place, it seems reasonable if we are to state fatigue in terms of sensations at all, that we shall find some sensory origin for such sensations. The argument frequently advanced that there are functional nerve endings and possibly receiving end-organs in the central nervous system itself that mediate these sensations seems somewhat fantastical and at least out of place until we have exhausted the more probable sources. It is not necessary to argue that the sensations actually originate in the nervous system to be able to say that the nervous system contains the place at which the first functional shift takes place in fatigue phenomena. We may say with Sherrington and others that the blocking is due to actual changes in the synapses, the critical functional points for nervous impulses according to this author. The fatigue sensations and the decreased ability in activity, as shown in the muscles and in the increased mental effort, would still be concerned in breaking up the 'reflex' and 'sensory-motor' arcs.

Mental fatigue is of peripheral origin. If we limit this thesis to the 'sensations of fatigue' the statement would probably stand at the present time without much challenge. The majority of observers are willing to assign the origin of all sensations that can be isolated in any satisfactory manner to a sensory-motor control, that, in part of its neural pathway at least, lies outside the central nervous system. In fatigue and kinæsthetic sensations, these sensory endings are gradually becoming more and more definitely located as our knowledge of the anatomy of the body enlarges. In fatigue phenomena and elsewhere, it seems less and less acceptable to assign to the nervous system *intra se* functions that seem to be in all other sense fields partly within the periphery. This is notably true in the highly detailed character of the analysis of eye movements that has led us to locate in the muscle coördinations many things that were previously held to be mental forms of an internal or metaphysical nature. So, for the fatigue that comes with a definite sense province, a special locus is gradually appearing as the analysis of the situation becomes clearer.

Even allowing that fatigue may possibly be a subnormal neural condition that is referred outward from the centers, we would still be compelled to assign some peripheral location to it. This holds good of 'referred pains' as well, and in fact the physician uses them merely as indications that point toward the real seat of the difficulty. In fatigue sensations he has a still better right to seek the seat of the difficulty in the periphery than at some central point, since here we know that certain parts are functioning and, in the majority of instances, we know exactly where those parts are located. This can be seen to apply without much difficulty in muscular effort, but it has not usually been considered as clear in the case of mental effort.

Two further points lead us to make the neuro-muscular hypothesis with reference to mental effort and the sensations accompanying it at all its stages. The first is that mental activity is not something superadded to the unconscious activity of an isolated muscle and differing in kind from it. So far as we know all conscious activity is accompanied by neural action, and in all organic life other than man we do not hesitate to assign definite correlations to the activity of neural and muscular tissue. 'Free imagery,' if there be such, would certainly be a distinctly new fact from the biological standpoint and constitute the entering wedge for any epiphenomenal doctrine of mind-body relationship.

The second point that leads us to make a peripheral statement of fatigue, both as a sensation and as a functional change, is connected with the close relationship that seems to exist between established motor coördinations on the one hand and types of imagery and mental processes on the other hand. There seems to exist in the naïve mind and in the experience of the psychologist as well, certain well-defined situations that as often as they appear in consciousness are recognized by the individual experiencing them as being his, *his* way of reacting, *his* way of feeling toward the situation that has arisen, *his* method of attending to the process from without, *his* way of getting rid of the offending situation, and so on throughout the gamut of experiences. Ordinarily this peculiar subjective province has been bodily assigned to the aggregate of things called the 'feelings,' or more accurately the affections. But the analysis has gradually been pushed into the realm of these feelings and the so-called feeling mass seems to break up into certain definite sensation groups with their attendant pleasure-pain correlates. As regards these sensation groups we can, I think, push the analysis a step farther and show in some instances exactly what constitutes such a group.

Another relationship is brought out in the feeling of having experienced a situation at some previous time, so-called similarity feeling, or relational feelings. Dr. Dewey's (24) analysis of the feelings of effort starts us off in this direction. Effort becomes the conscious formation of a plan of action that has for its conscious basis the imagery of the memory processes both reproductive and productive. In addition it involves a very definite set of sensations arising from the muscles that were previously involved. We are here dealing with what seem to be real 'attitudes.' The major portion of the situation is made up of previous muscular coördinations and their correlated sensations and perceptions. This usual and habitual group finds its expression in what any observer finds to be a familiar feature in the reactions of our illustrative person, and which is recognized by the experiencing individual himself in certain introspective situations as his own way of responding and recognizing.

It seems to us that the control test for the analysis of this type of experience offers itself in the fatigue experiment. Here we can with ease detect at various times the shifts in the controlling tensions, the gradual elimination of some, with the rise of others, till we suddenly find by a backward comparison that the whole field of control has shifted to an entirely different locus and that the attendant mental processes are something other than they were the few moments previous. We are dealing with the familiar shift of attention, in such a description. If the word 'attitude' is to have any meaning in psychology it must mean a group of fully coördinated elements that can find their locus in a common source. It seems anything but illuminating to find the term 'attitude' applied to such a heterogeneous mass of things as pleasure, pain, sensation states, interest, attention and so on, without seeking to find a common locus for the rise of these states (48). We should designate this common locus as the changes in strains and tensions arising from the musculature of the body.

Summary.—These motor attitudes, including in the term both sides of the process, the physiological and the psychological, are shifted by a special mechanism that presents its phenomena on the physiological side in three forms—inhibition and coördination, 'negative induction' or so-called muscular fatigue and fatigue proper, where the functional activity of the organism has diminished because of the attainment of a limit in the expenditure of stored-up energy. On the psychological side we may find three possible shifts in attitude that approximate these to some extent but not necessarily exactly, the reason being apparent in the differing functions assigned the two general fields. These shifts are produced by changes that are quite definitely 'in consciousness' in the formation of the focal points in the attention process. The first transition in attitude is made by those conscious correlates of the inhibitory and coördination processes that are called the 'feelings of effort'; the second by the 'sensations of fatigue' that are probable accompaniments of the process denominated by Sherrington 'negative induction.' As a special case, we have the conscious shift known as exhaustion, where the 'feelings of effort' have given place to their direct opposite. Consciousness expresses this situation in the absence of both 'efforts and strains,' and especially in the distinct loss of the preceding conscious process. In the third place, attitudes are modified and shifted by changes in stimulation, shifts from the sensory side of the process (adaptation and sense organ fatigue). The primary functional coördination for all these would be the neuro-muscular relationship residing in the proprio-ceptive or deep-lying muscle systems. The secondary functional factors would involve the two divisions of the externally receiving system, the extero-ceptive and the intero-ceptive, both mediating sensory elements that in the latter are as yet psychologically unanalyzed 'groups.'

Sense stimulations that arouse sensations of fatigue are closely allied to the stimulations that usually set off habitual responses and therefore fail in many instances to get into consciousness at all except in the retrospective way in which we seek for the stimulus that begins habitual activity. This experience is so ordinary in its manifestations that its relation to the problem of fatigue has usually been overlooked, and as psychologists we have been accustomed to look for the cause of the inattention and restlessness in images and external stimulations. As a habit, then, fatigue constitutes a highly specialized form of protection for the organism and enables the normal individual, who has not converted this habit into a means of avoiding effort, to gauge his activity within very definite limits.

This generalization is quite comparable to the situation described in part by Claparède[1] as leading to the appearance of the instinct of sleep. Allow the sensations of fatigue to arise undisturbed by a necessary end, the idea of a task that is incomplete, and the instinct that is most closely connected with it appears. The introduction of the feeling that the work must be done, together with the emotional element that accompanies this conflict of the biological and the psychical, causes a new condition to arise, which is psychical fatigue.

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[1] Both Féré and Joteyko have argued the biological significance of fatigue at some length; its correlation with the phenomenon of sleep has been mentioned in some detail by Claparède.

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Studies from the Johns Hopkins Psychological Laboratory

No. 2

The Determination of the Position of a Momentary Impression in the Temporal Course of a Moving Visual Impression

BY

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The Johns Hopkins Studies in Philosophy and Psychology

No. 3

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THE DETERMINATION OF THE POSITION OF A MOMENTARY IMPRESSION IN THE TEMPORAL COURSE OF A MOVING VISUAL IMPRESSION.

I. Historical Survey.

Herbart originated the use of the word 'Complication' to designate the conjunction in consciousness of dissimilar impressions. Wundt, following Herbart's usage, applied the term 'Complication Experiment' to the specific investigation of the perceived temporal relation of a single impression of one sense to a continuous series of impressions of another sense, into the course of which it is suddenly introduced.[1] For the continuous series of impressions Wundt employed visual stimuli, and for the disparate impression an auditory one. The general conditions of Wundt's experiment present a situation analogous to that obtaining in the so-called 'eye and ear' method of the astronomers, to which it owes its inception; and his research presents, under conditions which subject it to accurate psychological measurements, a problem similar to that with which the astronomers were early confronted.[2]

In the use of the transit instrument, the field of view is divided vertically by a series of equidistant threads, the central one of which represents the meridian. The astronomer, who wishes to note the moment of a star's transit, *i. e.*, passage across the meridian, observes the time given by a clock beating seconds. Then, his eye at the telescope, he counts the beats of the pendulum, to determine the second at which the star coincides with the meridian line. Since it seldom happens that the star is coincident with the line at the precise moment of the pendulum stroke, the additional fraction of a second is estimated by noting the position of the star at the stroke just preceding and at the stroke just following its passage across the meridian-line, and "dividing the time as the meridian-line seems to divide the space." Notwithstanding that a rigid control attempted to eliminate errors from every possible source, discrepancies invariably occurred in the computations of the time at which the star crossed the meridian, owing to the personal difference between various observers, the error frequently introducing a variation of more than a second in the readings. Bessel, who first noted this factor, gave to it the name of 'personal equation.' The analogy of the eye and ear method to the complication experiment will appear from a consideration of Wundt's procedure.

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[1] WUNDT, *Grundzüge der Physiol. Psychol.*, 4th ed., ii, pp. 401–; 5th ed., pp. 67–86.

[2] For a historical account of the "personal equation," see SANFORD, *Amer. Journal of Psychol.*, vol. ii, pp. 3–38, 271–298, 403–430.

The apparatus which Wundt first had constructed for the purposes of his experiments, he called the 'complication-clock."[1] It consists of a graduated circular dial, placed vertically and traversed clock-wise by an index-hand. The mechanism is that of a large clock, run by a weight, and having a fan-regulator for maintaining constant velocities, which however, may be arbitrarily varied within limits. It is so constructed that a bell-stroke may be produced coincidently with the alignment of the index-hand with any chosen division of the scale on the dial, the point selected on the scale being unknown to the observer. The index-hand of Wundt's apparatus was 25 cm. in length, equal to the radius of the dial measured to the proximal end of scale divisions; *i. e.*, the index-hand extended outward only as far as the scale divisions, so that in rotating it did not pass over them but moved by their inner ends.

Comparing now the elements in the mechanism of the complication apparatus with those of the eye and ear method of the astronomers, it is seen that the equidistant threads dividing the field of view of the transit instrument correspond to the series of scale divisions on the complication dial, that the star is represented by the index-hand, while to the pendulum-stroke of the clock beating seconds corresponds the auditory impression entering regularly into the course of the series of visual impressions of Wundt's complication apparatus. From this comparison it is clear that the errors in the estimated time of coincidence of star and meridian are analogous to the time-displacements occurring in the attempt to judge the position on the scale at which the passage of the index-hand is synchronous with the bell-stroke, as presented in the complication experiment. Later, Wundt substituted for the complication clock, with its uniform rate of rotation, a form of apparatus with index-hand actuated by the swinging of a pendulum, with a view to introducing the factor of varying acceleration.

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[1] For a complete description of Wundt's complication-clock, see GEIGER, *Philos. Stud.*, vol. xviii, p. 349.

The second form of apparatus, as distinguished from the complication-clock, he called the complication-pendulum. As in the complication-clock, the position of the index-hand (with regard to the scale), to which the bell-stroke corresponds, may be varied at will; but there is a difference in the movement of the index-hand, in that, as it now depends upon the swinging of a pendulum, it no longer rotates as before, but oscillates back and forth across the upper half of the dial, the instrument being so contrived that the bell-stroke occurs only when the index-hand is moving in the direction corresponding to that of the hands of a clock; *i. e.*, from left to right. The pendulum is at the center of its swing when the index-hand is in alignment with the upper vertical radius of the dial, to the right and left of which, depending upon the direction of movement, it is accelerating or retarding, in accordance with the law of harmonic motion. The radius of the scale and of the index-hand, which traverses the inner margin of the scale as in the original form of apparatus, is 17 cm. The scale is a half-circumference in extent.[1]

Wundt was himself 'subject' throughout his experiments. When using the clock-machine, he stood in front of the dial, which was concealed by a wooden screen until the index-hand had attained the desired speed. He adopted no rigid method of observation, but in a manner as free from prejudices as possible, awaited the moment at which, after a sufficient number of revolutions, he felt secure in naming the position upon the scale at which the index-hand appeared to be at the stroke of the bell. He noted this point; then, stopping the machine, compared his judgment with the actual position of the bell-stroke, which was determined by slowly rotating the index-hand until the bell-stroke occurred. Then he fixed the position for the next experiment, making the change carelessly, so that he did not know the new position. The rates of rotation which Wundt used varied between one revolution in a little less than 2 secs. and one revolution in a little more than 8 secs. When the index-hand revolved at a rate much slower than 8 secs., its position was clearly visible; when much more rapid than 2 secs., it was not possible to obtain a judgment.

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[1] For full description of pendulum-apparatus see WUNDT, *Physiol. Psych.*, 5th ed., vol. iii, p. 80.

In the outcome of his enquiry, Wundt found that correct judgments were the rare exception; that is to say, that it was seldom, and then apparently by accident, that the subject's *feeling* of the simultaneity of the sound with a given position of the index-hand agreed with the *actual* simultaneity of the two impressions. The subject selected a position on the scale at which the index-hand arrived either after the sound occurred or before it occurred. That is, in the former case, the sound impression was as if too tardy, the index-hand having already passed the assigned position, whilst in the latter case, the sound impression was as if too premature, the index-hand not yet having had time to reach this position before the sound was perceived. These two classes of error Wundt called arbitrarily *positive* and *negative;* positive errors occurring when the assumed position of simultaneity is later than the real one, and negative errors when the assumed position is earlier than the real one. Wundt found, contrary to the natural expectation, that negative errors were largely preponderant, it being the prevailing tendency to anticipate the sound, as it were.

Among the factors modifying the results, Wundt regarded the influence of speed as of chief importance. This is manifested in the difference in the quality of the error, according as the rate of rotation is slow or rapid; the negative errors predominating with slow speeds, whilst with greater velocities the positive errors prevail. At a certain point between the extremes of speed the average error becomes zero. This indifference point Wundt found to lie within the range of rotation-rates from 5 to 2 secs. With the complication-pendulum, with which he operated in the subsequent experiments, Wundt obtained results which agreed in the main with the preceding. The especial feature which Wundt noted in the use of the pendulum apparatus was a characteristic tendency to more negative errors if the bell-stroke occurred during a phase of acceleration of the index-hand (as was the case when moving from the left upward toward the median-line of the dial) and the tendency to more positive errors when the bell-stroke occurred during a phase of retardation of the index-hand, (as when moving downward from the median-line to the right-hand limit of the scale).

W. von Tschisch, who succeeded Wundt in experiments upon the complication problem, also made use of the pendulum machine.[1] He too was 'subject' for his own experiments. His results, in the main, corroborated Wundt's, the negative errors becoming less negative with increasing speed; but his average absolute errors were invariably negative, when experimenting, as Wundt, with a single complicating stimulus. The rates of oscillation of the pendulum, and therefore of the index-hand, used by von Tschisch, were one oscillation in 2 secs., in 1.5 secs., and in 1 sec.

The chief interest of von Tschisch's investigations consists in the substitution of other complicating impressions for the auditory impression of Wundt, and in the combination with sound impressions of other stimuli and of these in varying ways among themselves. For example, employing a touch-impression instead of sound, the results showed no noticeable change. However, where one or more disparate impressions were simultaneously combined with sound, the negative error was made smaller, and the error shifted progressively in a positive direction in proportion to the number of disparate stimuli which were thus combined. When three disparate complicating stimuli were employed, the error was positive.

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[1] Ueber die Zeitverhältniss der Apperception einfacher und zusammengesetzer Vorstellungen. *Philos. Studien*, ii, pp. 603–634.

The occurrence of two disparate impressions within the same sense realm, but differing in quality, as the stroke of a bell and that of a hammer, have the like effect of diminishing the negative error. Von Tschisch further observed that the diminishing effect upon the negative errors exerted through adding to the number of momentary simultaneous impressions, was far less under conditions which admitted of an association between the impressions in question; *e. g.*, stimuli upon closely adjacent areas of the skin, wherein the touch impressions are united in a single space concept. If instead, widely separated areas of the skin be stimulated, the subject reacts as if disparate impressions were employed.

The next investigator in the field of complication-work was C. D. Pflaum.[1] He too used the pendulum form of apparatus. He experimented on himself and on two other subjects. His results concur with those of the two former Leipsic experimenters, *i. e.*, they show a preponderance of negative errors, which with increasing speed manifest a positive trend. The rates of pendulum oscillation employed by Pflaum were 2, $1\frac{2}{3}$, $1\frac{1}{2}$, and 1 seconds per oscillation.

It may be said here that all of the above experimenters are in harmony in respect to the essential returns. All are in accord as to the importance of the factor of the rate of rotation in determining the quality of the errors, the tendency to negative errors predominating with the slow speeds; and the gradual increase of positive errors with increased velocities. The experiments of both Wundt and von Tschisch number in the thousands, those of Pflaum number several hundred.

We next come to consider the studies upon the complication phenomena which were carried on in the Harvard Laboratory by J. R. Angell and A. H. Pierce.[2] These investigators resorted again to the method of uniform velocities and for their purposes constructed an apparatus similar in principle to the complication clock of Wundt. An ordinary kymograph served as the motive power. The chief difference between it and the older instrument consisted in the substitution of a sharp click for the reverberating bell-stroke, and of a dial which was but a portion (about $\frac{1}{6}$) of a circumference. The arc of the circle represented by the dial was below the center, its middle point lying on the lower vertical radius of the circle, so that as the index-hand revolved in the direction of the hands of a clock, its direction, when crossing the dial, was from right to left, and the index-hand, when approaching the center of the dial, moved in a downward course, just the opposite of the course of the index-hand in the complication pendulum, in which the arc of the circle traversed was above.

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[1] Neue Untersuchungen über die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Complicationspendel. *Philos. Studien* xv, pp. 139–148.

[2] Experimental Research upon the Phenomena of Attention. *Amer. J. of Psychol.*, iv, pp. 520–541.

The index-hand which Angell and Pierce employed was much longer than the index-hand of the Wundt machine, measuring 47 cm. from center to tip. The complicating stimulus consisted of a sharp click produced by an electric hammer. The circuit through which the hammer was actuated was closed by a wire fastened to the index-hand and dipping into a mercury cup behind the scale. The position of the mercury cup along the scale was variable, and hence the sound could be fixed for any desired position of the index on the scale. In method, Angell and Pierce departed from the hitherto customary plan of leaving the subject quite free and untrammeled as to his procedure and adopted a usage which was, in their own words: "to follow the pointer on its first revolution until the sound is heard, when we attempt to stop the movements of the eyes instantaneously. The point on the dial thus attained is made the basis of operation for the next revolution, when any seemingly needed correction is made." They chose three rates: a slow, a medium, and a rapid rate, representing one revolution in 6.06 secs., 3.6 secs., and 1.2 secs. respectively. Angell and Pierce had presumably but few observers, as their experiments amounted to a comparatively small number. Their results show a wide divergence from those of the preceding German experimenters. In the first place they find on the whole, in contradiction to Wundt and his followers, a preponderance of positive errors appearing in the results of each subject toward the end of their series of experiments, which indicates in the light of their interpretation the presence of a hitherto unrecognized factor, namely, that of practice. In the second place their results were further at variance with those of the preceding investigators in that they discovered no influence of velocity. The factor of varying acceleration did not come into court, since they worked with uniform rates of rotation.

In a comparative survey of the results of the two sets of investigators and of the conclusions reached by them with regard to the basic factors operative in producing the errors in the complication experiment, it appears that the German experimenters find a preponderance of negative values, while contrariwise the Americans find that the tendency to positive errors is the prevailing one, at least after practice. Further, the German investigators find that the negative values predominate with the slower speeds and the positive with the faster. With them no influence of practice is manifest. Angell and Pierce on the other hand deny the influence of speed in determining the quality of the judgment, but emphasize the influence of practice, the effect of which is gradually to shift the errors from the negative direction toward the positive. So that the conclusions of Angell and Pierce, as to the result of their inquiry were: influence of practice and no influence of speed, and of the Leipsic experimenters: influence of speed and no influence of practice. As said above, Angell and Pierce used constant rates of rotation, so that the question of the influence of varying acceleration inseparable from experiments with the pendulum apparatus did not enter into their determinations.

Such was the conflict between the returns of the Leipsic and the Harvard investigators until M. Geiger, working in Wundt's laboratory, attacked afresh the problem of complication.[1] This investigator reverted to the original type of apparatus, represented by Wundt's complication clock, but he departed from Wundt's procedure, in abandoning the use of the wooden screen for concealing the apparatus in the intervals of experimentation, preferring simply to disconnect the sound-stimulus instead, as the repeated raising and lowering of the screen tended, in the opinion of this experimenter, to confuse the subject and vitiate the results. He also substituted for the bell a sharp hammer stroke, as the reverberations of the bell-stroke failed to satisfy the requirements for a strictly momentary auditory impression, and so did not fulfill the conditions essential to the experiment.

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[1] Neue Complicationsversuche. *Philos. Studien*, xviii, pp. 347–436.

Since in the beginning of his work Geiger's method was directed by the purpose of a series of experiments serving as a control upon the results hitherto attained, he at the outset sought to eliminate the coefficient of individual peculiarity and to this end enrolled seven observers for the conduct of his experiments, a larger number than had been employed by any previous experimenter. With a view to isolating the influences of practice and of speed as factors determining the quality and magnitude of the errors, Geiger began with each observer with a speed of one rotation in 8 secs., and with successive sittings progressively decreased the time of rotation by half-second decrements to 1 sec., and finally to 0.9 sec., the extreme of speed to which his apparatus was adjustable. He then reversed the proceeding, beginning with a rotation-rate of one revolution in 0.9 sec., increasing the time to one revolution in 1 sec. and from here upward by ½ sec. increments till a rotation-time of 8 secs. was again reached.

Geiger, whose work, as indicated, is primarily a control upon the conflicting results of his predecessors in the field, reconciles, in the main, the contradictory aspects presented by the conclusions of Wundt and his pupils on the one hand and those of Angell and Pierce on the other. He shows that both factors are operative—the influence of speeds and the influence of practice.

Having proceeded as above described, Geiger constructed a curve that traces the effect of his first series of experiments, that namely in which he used gradually increasing rates of speed, to illustrate the joint influence of practice and of increasing velocity—the two determinants going hand in hand in such a series. The curve thus derived represents a gradual progression from a markedly negative beginning to a termination upon the positive side. Now if the conclusion of the Leipsic experimenters is correct and the positive tendency in the above series is due to speed alone, then the tracing of a second curve drawn from comparison and derived from a precisely contrary proceeding, *i. e.*, setting out with the fastest rate and gradually passing over to the slowest, ought to proceed in a direction just the reverse of the former, and represent an approximate retracing of the first curve in the opposite direction. But if, on the other hand, the assumption of the American observers is correct and practice alone is the decisive factor, the continued practice in operation after reversing the direction of change of velocities ought, on the contrary, to be represented by a curve that mounts still higher in the positive direction, continuing the course of the original curve. Now the curve actually resulting in the case of each of Geiger's subjects is found to be consistent with neither assumption, but is clearly the resultant of both forces —namely the influence of speed and the influence of practice; for the second curve is neither a reversal nor a continuation of the first, but exhibits a trend which in the main fluctuates between the influences of the two factors, the tracing for some observers tending feebly toward the positive, in others as faintly toward the negative side. True, there is a slight partiality toward the latter course, since in both curves the influence of speed is uniformly operative throughout, while from first to last the influence of practice is a decreasing factor and with the commencement of the second curve its efficacy has considerably waned.

With one of Geiger's subjects the influence of speed is shown in isolation. Here the succession of speeds proceeds irregularly, yet the rise of the curve coincides with the increased velocities. Further the influence of practice is manifest in individual experiments in which it happens that speeds which give an error of four or five scale-divisions, later reduce the error to nil. According to Geiger, the influences exerted by practice upon the errors are chiefly two. 1. With constant velocities increasing practice makes the errors constantly less negative or more positive. 2. Practice causes the maximal negative error (or minimal positive error) to occur at a speed greater than that at which it previously occurred.

From the above experiments, it is evident that Geiger's results are in full accord neither with those of the Leipsic nor with those of the Harvard observers. Geiger seeks to explain the discrepancy between Wundt's results and those of Angell and Pierce and himself with regard to the influence of practice, on the ground of the difference in apparatus employed. The pendulum-apparatus which Wundt used precludes by reason of its constantly varying speeds the possibility of effects of practice at any speed, while, on the contrary, the rotation device of Angell and Pierce and the complication-clock of Geiger favor the introduction of such effects through the uniformity in the movement of the index-hand. The explanation of the divergence of Angell and Pierce's results from Wundt's and his own in their failure to detect any influence of speed may be more fittingly stated after the consideration of the other constituents which Geiger includes in the sum of factors entering in to determine the results.

As a factor influencing the magnitude and the direction of the errors, Geiger lays stress upon the difference in the manner of observing characteristic of different individuals and accordingly distinguishes two types of observer, which he calls, for convenience, '*naive*' and the '*reflecting*,' and names the corresponding methods, the 'index-hand' and the 'scale' method of observation. Characteristic of the 'naïve' or 'index-hand' method is the passive, quiescent, leisurely attitude of the subject. Quite indifferently he permits the impression of the *region* of probable simultaneity of sound and index-hand to arise within him, and later, with successive rotations, he selects, within this region, or rather there selects itself, as it were, the position at which the index-hand and the stroke are synchronous. The observer of the 'reflecting' type on the contrary is tense and alert. He is bent upon catching as quickly as possible the exact position of the index-hand at which the sound enters. He does not wait composedly until, after several rotations of the index-hand, the apparent region of simultaneity is established, but seeks to determine its position upon the scale at the first entrance of the sound. He now holds strongly to the scale-division thus selected and with the next revolution fixes it by the reading on the scale and then tries further to confirm his choice with the succeeding revolutions, or if unsuccessful, to make the needed correction, depending upon whether the sound appears to come before or after the point selected. Thus it is seen that careful thought and more or less of computation enter into the determination of the resulting judgment and give the cue for designating it as of the 'reflecting' type.

The different attitudes of the two types of observers toward the judgment made upon the first revolution of the index-hand is significant. One of Geiger's subjects who was of the 'reflecting' type, records that if he succeeds in getting a judgment on the first rotation, he has a feeling of security as to his determination, but Geiger himself, whose method was 'naïve,' finds the first judgment (the judgment obtained upon the first rotation of the index-hand,) relatively of no importance for his subsequent determination.

It is noteworthy that the 'naïve' subjects were able to observe according to the 'reflecting' method when directed to make the attempt, while the reverse was not readily the case. A further point of differentiation between the opposed types is to be seen in the importance of the rôle which accidental fixations of the attention play in 'naïve' judgments, as evidenced by the change of reading with successive rotations—each such change ranging over an interval of as many as 3 or 4 scale-divisions.[1]

Geiger's results show that for those of his observers whom, upon the basis of their method of procedure, he had classified as 'reflecting', the two curves above mentioned lie for the greater part upon the positive side, while those belonging to the 'naïve' type presented curves which lie upon the negative side. With the subjects in whom Geiger found neither type characteristically presented, but in whom the method consisted in a mingling of the two procedures, the curves follow courses which in general are intermediate to those of the distinctly discernible types. Since in the light of the method of experimentation employed by Angell and Pierce their procedure clearly corresponds to the 'reflecting' type described by Geiger, he refers the preponderance of positive results shown by them to their experimental usage and traces to this source the causes of the discrepancy between the returns of these experimenters and those of Wundt. However, in seeking to base an explanation of the negative errors of the Leipsic experimenters on a reverse procedure, the clue did not prove to be the correct one, for in cases in regard to which information was obtainable, Geiger learned that the procedure adopted by these investigators included both methods, so that he was obliged to seek for other explanations for the peculiar results of these experimenters, and finally ascribed the difference of result to the different apparatus used. For obviously, owing to the difficulties attendant upon the inconstancy of the velocity which is a condition of the pendulum apparatus, the factor of practice is eliminated, and the two methods of observation are no longer characteristically present, but (Geiger maintains) lose their specific qualities and merge more or less into one another. We shall return to the question of temperamental types in a further connection.

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[1] The scale was divided into 100 parts

It has been shown that increasing speeds of rotation increase the positive tendency of the errors. But increase in the rate of movement of the index-hand goes hand in hand with increase in the rate at which the momentary or disparate impression occurs. Hence the question arises: To which of the two factors is the result attributable? In the 'eye and ear' method of the astronomers it has been observed in some cases that positive errors increase with the more rapid course of the star[1] and in this case of course the rapidity of the disparate impression (clock-tick) is unchanged. It had also been shown by Bessel that altering the rate of the clock beat alone might change the error in the observation of the star's transit. Geiger attempted to investigate the influence of altering the rate of the successive strokes of the sound alone, the visual impressions occurring at the same rate. This he did by taking a series of experiments in the usual way and alternating with a series in which the hammer-stroke occurred only at alternate

[1] Sanford, *Op. Cit.*, pp. 287–8.

revolutions of the index, so that the rate of auditory rhythm was decreased to just half the original rate. In his account of this variation of his experiments, Geiger is unsatisfactory. He says at one place that the effect of omitting the alternate strokes is a greater positive tendency, at another that it is a less. Further, his final statement is not in accord with his tables. This method of determining the influence of the slower auditory rhythm is inadequate at best, for in thus introducing two revolutions of the index-hand for each auditory stimulus, Geiger arbitrarily neglects the factor of the alternate blank rotation of the index-hand and of the possible effect of this upon the attentive process (in deflecting the judgment to one side or the other). Moreover there is no certainty that the actual rhythm is changed at all by this method, since the interval between two strokes at the slower rate may be broken up into two attention-periods, corresponding to the periods of the fast rate.

An interesting point first brought out by Geiger is the factor of positional error, dependent upon the position of the index-hand at the moment of the auditory stimulus, in one case on the right hand side of the scale, in which it has a downward direction, and in the other case on the left hand side, in which position the direction of its movement is upward. The experiments which Geiger undertook to test the influence of positional error show with remarkable regularity that the deviation of the average error upon the right side of the scale from the general average error is in the positive direction, and upon the left side in the negative direction, the maximal negative error in the latter case lying toward the beginning of the upward course of the index-hand, while the positive influence upon the errors tends to a more uniform distribution upon the corresponding side of the scale.

Von Tschisch earlier called attention to this difference in the tendency of the errors upon the opposite sides of the dial, but he attributed the source to the acceleration of the index-hand, characteristic of the pendulum-machine, when moving upward as on the left side, and to the retardation when moving downward as on the right. Apparently von Tschisch was upon the wrong track, since Geiger observed the influence of the same factor with the uniform velocities of the complication-clock.

Wundt and Pflaum, who, as has been said, made use of the pendulum machine, also performed experiments which showed the stronger negative influence of the upward moving index-hand when the upward movement corresponded to the phase of acceleration, or the stronger positive influence of the downward movement, when this coincided with the phase of retardation of the index-hand. But these observers failed to discriminate severally the two factors of which their determinations expressed the resultant. The attempt at the differentiation of the factors here involved was undertaken by a later investigator who will be mentioned below. The problem with which Geiger was concerned had to do with the simpler conditions presented in the clock machine, in which the factor of change of acceleration was eliminated, allowing the isolation of the influence of the different directions of motion on the lateral halves of the dial.

In order to rule out a possible defect of the apparatus which might operate in such a way as to cause an actual difference in the movement of the index-hand upon one side or the other, Geiger obtained an inverted image of the dial by interposing a telescope between it and the observer, the observer's visual field being exactly coterminus with the dial. Thus the uppermost and lowermost points being interchanged, the index-hand now moved in a downward course toward the point which before was the top of the dial and in an upward course toward the point which before was the bottom. That is to say, the image of the hand moved down when the real hand moved up, and *vice versa.* Notwithstanding the relative reversal of the positions of the dial in regard to this upward and downward direction of movement of the index-hand, the results were unaltered—the errors being more negative on the side of apparent upward movement, or more positive on the side of apparent downward movement, thus clearly excluding the possibility of irregularity in the mechanism of rotation as a cause of the difference in errors upon the two sides of the scale.

Geiger subjected to a similar test the influence of a possible predilection of the subject for the right or left side in causing the difference in the quality of the judgments, by reversing the two sides. This he did by having the subject observe the image of the dial reflected in a mirror. Again the results were substantially unchanged, showing that the direction of rotation from left or right was inconsequential.

Finally, Geiger proceeded to test the influence of the peripheral factor of eye-movements occurring concomitantly with the upward and downward movement of the index-hand. To this end he experimented in a darkened room and by replacing the original form of index-hand and scale-divisions with slits in a black surface and made visible by light entering from behind, he was able to observe the relative breadth of the after-image obtained during movement of the index-hand in the two directions. The results showed an after-image of greater extent upon the side of upward than upon that of downward movement, thus introducing a condition favorable to more negative readings upon the side of the ascending direction of the index-hand, as compared with those of the descending.

While Geiger disentangled the elements of acceleration *versus* retardation upon the one hand (as presented in pendulum apparatus) and of upward and downward movement of the index-hand upon the other and traced the source of the latter to the relatively easier downward than upward movements of the eyes, Klemm working with the pendulum,[1] further isolated the two factors by alternately transposing the conditions of experimentation in such a manner, that the upward movement of the index-hand of the complication-pendulum was now coincident with retardation of the velocity, while conversely the downward movement was accompanied by an acceleration of the index-hand. This interchange of the relations of acceleration and retardation to the upward and downward movements of the index-hand, Klemm effected by inverting the medial position of the index-hand. This being in the original apparatus above, it was now below the center of the scale (the index-hand of Klemm's apparatus described nearly a complete circle), and therefore as the index-hand now approached the mid-point of the scale it moved in a downward course, with an increasing velocity: and as it left the mid-point it moved in an upward direction with a decreasing speed.

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[1] Versuche mit dem Komplicationspendel nach der Methode der Selbsteinstellung. *Psychol. Studien*, ii, pp. 324–357.

Now upon comparing points on the upward path of the index-hand in the transposed arrangement with similarly situated points on the downward path in the original arrangement, and *vice versa*, thus entirely excluding the influence of acceleration or of retardation, he found that the preponderant negativity still obtained upon the side of ascent; and upon a comparison of points upon the paths in the two arrangements, which points were identical as regards direction of movement and velocity, but at which was presented in the one case a phase of acceleration and in the other a phase of retardation, Klemm still found coincident with the phase of acceleration a greater negative tendency of the error as compared with the tendency occurring with retardation.[1]

The especial interest of the experiments undertaken by Klemm lies in the altered method of procedure whereby the observer himself places a given mark in the position on an otherwise unmarked dial, at which the index seems to be when the sound enters. (The fundamental apparatus he employed is a modification of the original Wundt complication-pendulum.) In essentials the auxiliary device which this investigator used consists of a mirror, placed in front of the observer, between him and the dial. A circular opening through the mirror permits the observer to follow the movement of the shaft of the index-hand upon the dial, while the end of the index-hand, together with the scale is concealed from view by the mirror. The observer looks through a small circular opening in a screen, behind which he is seated. Upon the face of the screen opposite the mirror is a white annular surface the size of which corresponds to the graduated circumference of the dial, which

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[1] It will be pointed out in Pt. III that Klemm's results on this point by no means indicate that the differences are really due to retardation and acceleration.

(the mirror being placed in a position equidistant from the dial and the white annular surface), is directly replaced by the reflection of the white ring—it being understood that the line of vision is perpendicular to the plane of the dial, reflected ring, mirror, its annular opening, and the hole through which the observer looks, at their centers. A second index-hand, called the marker, which may be moved radially over the white ring by means of a lever placed correspondingly to it upon the back of the screen, is thus under the control of the observer. With this arrangement therefore the observer who, through the opening in the mirror, follows directly the movement of the index-hand, may indicate its position in relation to the complicating stimulus, by adjusting the reflected marker to the position in which it is in alignment with the index-hand at the stroke of the sound-impression. Klemm used only two rates, 1.2 sec. and 2.1 sec. per oscillation. His farther problem was the determination of the width of the 'Simultaneitätsbereich;' the simultaneity zone, or region within which all positions are practically simultaneous with the discrete stimulus; and its relation to the average deviation or error. The main point brought out in this regard is the extreme variability of the region in question.

A simple complication-apparatus was devised by H. C. Stevens[1] through the use of a bell metronome. A piece of white card-board is fastened to the instrument in such a way that the pendulum oscillates in front of it. The card-board presents the arc of a circle, the radius of which is equal to the length of the pendulum, scaled in 5° intervals. The position of objective coincidence of the bell-stroke and pendulum, may be ascertained as in Wundt's machine, by slowly moving the pendulum across the dial until the bell sounds. The method of Stevens' experiments with this apparatus requires that the subject shall at one time direct his attention to the sound, in which case the sound appears to come *before* the position of objective coincidence (negative error), and at another that he direct his attention to the position of the pendulum, in which case the sound appears to come after the real positions (positive error). This modification of method Titchener[1] calls the 'inversion' of the complication experiment, because in it the subject is not left unbiased but is expressly instructed to which member of the two simultaneously occurring stimuli he shall voluntarily direct his attention.

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[1] A simple Complication Pendulum for Qualitative Work. *Amer. Jour. of Psych.*, xv, 1904, p. 581.

T. H. Haines[2] took up the complication problem in a new way. His apparatus consisted essentially of a black disc on which were mounted at regular intervals white letters, which, by the rotation of the disc were brought in succession before the field of view of an observation tube. This series of visual stimulations was complicated with an auditory impression produced by the stroke of an electric hammer. The circuit for this hammer was completed through a mercury cup by a copper wire attached to a wooden arm on the rotating shaft which bore the disc.

Haines used various rates of succession of the letters and of the sounds, varying them together and independently. His results agree in part with the Leipsic rule of change of average error with rate, and with the Angell and Pierce rule of change with increasing habituation. The fact that no correction was made for the errors in the apparatus, which were probably large at certain rates, makes the interpretation somewhat insecure. The method, however, seems a promising one.

Certain observations were common to all the experimenters. All noted the significance of accidental combinations of the attention in determining the judgments. Certain scale-divisions seemed to exercise a peculiar fascination upon the attention, and Wundt noticed that the attention invariably held to the scale-divisions, even when the actual position of entrance of the sound was made to occur midway between them and in cases in which the scale-divisions were separated by a considerable interval. Wundt performed experiments in which he concealed from view all but a single scale-division, and setting the position of the sound at points on one side and the other, he obtained errors as great as $\frac{1}{4}$ sec. in the time-displacement. Geiger experimenting similarly obtained errors of as much as $\frac{1}{10}$ sec. It was also the general observation that a single revolution of the index-hand is inadequate to insure a conclusive judgment. For this it was necessary that a sense of rhythm in the succession of the momentary sound stimuli establish itself; so that several rotations were often requisite before the mind felt prepared to determine its choice. The difference in the number of preliminary rotations required, Geiger found to vary for his subjects between as few as three and as many as forty-three revolutions. Pflaum observed that the number of preliminary rotations required increased with the more rapid rates. If an increase occurred while the speed remained constant, it was an indication of the onset of fatigue.

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[1] *Psychology of Feeling and Attention*, p. 252.

[2] *Harvard Psychological Studies* 1906, vol. ii, pp. 309–348.

II. NEW EXPERIMENTS AND RESULTS.

In the work just reported the assumption has been implicitly and sometimes explicitly made that the time-displacement of the discrete stimulus in relation to the series of other stimuli is essentially a complication phenomenon, *i. e.*, that it depends upon the introduction of a disparate impression into the series.

In the paragraph of Wundt's Physiologische Psychologie introductory to his description of the complication experiment,[1] he applies the name 'time displacements' to such time illusions as occur when simultaneous sense impressions or sense impressions but slightly discrepant in time appear to be displaced in relation to each other in such wise that impressions which are in truth simultaneous are perceived as successive, or where a succession is actually present, the impressions are perceived as simultaneous or even transposed so that the earlier is perceived as the later and the later as the earlier. But Wundt insists upon the essential disparateness of the two impressions as the *sine qua non* of such displacements and there is in the text the obvious implication that this stipulation applies equally under the specific conditions of the complication experiment. "Such time-displacements," he says, "occur essentially between disparate sense impressions, that is, between visual and auditory, visual and tactual, and between tactual and auditory. They happen, when the impressions are within the same sense-realm, only when the impressions affect different individual organs, as the two eyes or ears, or positions upon one and the same organ which are widely removed from each other, for example, upon the skin of the upper and of the lower extremity. That stimuli upon the same ear or eye, or upon one and the same position of the skin should be temporally displaced with regard to each other, is, on the contrary, never to be observed."

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[1] 5th ed., p. 4.

Wundt then attributes the chief source of the phenomenon of time-displacements to the preference of the attention for stimuli received upon sense-organs in which the physiological transmission is the more rapid, claiming upon this ground, that the attention seizes upon an auditory impression more readily than upon a visual one, because of the conditions for the quicker reception of a stimulus in this sense organ.

He then argues that the sense organ in which stimuli are more favored by the attention is, *de facto*, the sense organ which will find most favor in the process of 'apperception.' It was primarily with a view to testing the validity of the above quoted statement of Wundt, and the explanation of time-displacements hinging upon it, together with the *a priori* assumptions Wundt adduces in support of it, that my experiments were undertaken.

To subject to a critical experimental test the question whether time-displacements may occur where the two impressions belong to the same sense organ and are received upon adjacent portions of it, I substituted for the disparate auditory stimulus entering momentarily into the course of the series of visual impressions as presented in Wundt's complication experiment, a momentary visual impression.

Before describing the technique of my procedure, I wish to call attention to certain features of the apparatus I employed, in which it possesses a distinct advantage over the instruments hitherto used.

As I have said, the dial of Wundt's machine was marked with scale-divisions. Those were clearly seen by the observer and by means of them he indicated his judgment of the position. In another arrangement, which Geiger adopted for the purposes of a variation of his experiment, the index-hand traverses a surface which is unmarked throughout its extent. Obviating the objections to which both these arrangements are open, a characteristic feature of my experiment is the introduction of the device whereby the subject may himself shift the temporal position of the discrete stimulus until it appears simultaneous with the alignment of the index-hand and a determinate mark or goal upon an otherwise undifferentiated dial. It will be recalled that Klemm employed the method by which the subject uses an adjustable marker to denote the position at which he judges the two impressions to be simultaneous, but by this method of self-fixation it is the marker which is brought to the position of entry of the discrete stimulus. This is doubtless a convenient method of locating the position of subjective simultaneity, and permits the determination of the limits of the region of simultaneity, but it does not otherwise alter the essential conditions of the experiment.

My apparatus had a semicircular dial, upon which appeared a single black mark (called the *goal*), placed at the middle point. The pointer moved over this dial and by this mark at uniform rates, while the temporal position of the discrete stimulus with regard to the series of positions of the index-hand could be varied at will by the movement of a lever. The subject was required to fix his eyes on the goal and vary the aforesaid temporal position, until it coincided with the alignment of index-hand and goal, starting alternately from positions much earlier and much later.

The advantage of the method I adopted is three-fold. In the first place it is to be preferred to the method of determining the position by the scale-divisions of the graduated dial, because it eliminates the random influence upon the attention of certain of the scale-divisions and concentrates the attention uniformly upon a single mark, which the index-hand is to reach simultaneously with the entrance of the disparate impression. Secondly, it takes precedence over the method whereby the subject selects the position in question upon an entirely unmarked dial, for the reason that there is in this case no mark which may serve the attention as a fixation-point to which it may attach itself as an aid to the subject in determining his judgment, or upon which he may depend for reference in designating subsequently the selected position, without involving the element of memory. And lastly and perhaps of chief importance among the features essential to the method to which my apparatus is adapted is the elimination of eye-movements around or across the dial. For here the arrangement is precisely the opposite of that introduced by Klemm, and instead of an adjustable marker being brought to the position of apparent simultaneity of the impressions, the momentary stimulus is brought, as it were, to the marker or goal at the instant of the arrival there of the index-hand—the discrete stimulus being thus made simultaneous with the alignment of the index-hand and the goal. Thus the goal at which the disparate impressions are to be brought simultaneously is the focus of interest. Upon this the eyes are steadily fixated, instead of having to shift constantly to catch the position at which the stimulus enters, as in the method of Wundt and others.[1] Finally, my method shows a further departure from the routines adopted heretofore (with the exception of Klemm's) in that it presents the factor of two opposite directions of approach, *i. e.*, from definite positions on the side, where the stimulus clearly comes before the alignment and from definite positions on the side, where alignment precedes stimulus, thus neutralizing in the total of the results of the two types the influences due to direction of approach.

My apparatus was constructed from a Schumann time-machine[2] in the following way. A metal disc M, 25 cm. in radius, bored through the center to fit the shaft S of the time-machine, was slipped over the latter and rested horizontally upon the pulley P. Resting upon the metal disc was a corresponding disc of white card-board *D*. Both discs rotated with the shaft and with the contact arm *T*, but the position of the card-board disc and therefore of the index-hand could easily be changed. Upon the card-board disc was pasted the index-hand *H*, which consisted of a black strip, 11 cm. in length, lying upon a radius and terminating at the edge of the disc. The strip was wedge-shaped, its apex at the edge of the disc being 2 mm. in width, and its base 14 mm. In the horizontal plane with the card-board disc was a card-board dial, presenting an originally rectangular surface, in one edge of which, equidistant from either end, was niched a semi-circle having a radius slightly greater than that of the disc and placed concentrically and nearly touching the disc. The disc therefore rotated in close proximity to the concave edge. From the center of this concave edge, a scale was drawn outward 75° toward either side, the scale-divisions, drawn lightly with pencil at 5° intervals, being only with difficulty visible to the observer. At the mid-point for a mark or goal *G* was pasted a strip of black paper similar to the index-hand, but having a length of only 6 cm., and width tapering from 1 cm. to 3 mm. This strip was placed radially, its narrowest end at the edge of the card-board.

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[1] While the apparatus employed by me is similar to Klemm's in the introduction of the method of self-fixation, it was devised entirely independently. My experiments had continued for some months prior to the appearance of Klemm's article, in which is presented a description of his device.

[2] *Zeitschr. f. Psychol.*, Bd. 17. S. 253. See Catalog of Spindler & Hoyer for cut.

The circular brass scale *N*, beneath the revolving parts of the time-machine was cleared of all impedimenta save the single contact-block which moves with the lever *L*. A mercury contact *R Q* was employed when working with a light-stimulus, and a metallic contact in the case of the sound-work.

A small Zimmerman sound-hammer placed directly beneath the center of the dial served for the auditory-stimulus. For the light-stimulus I used the flash of a Geissler tube *E*, 12 cm. in length, placed horizontally about 12.5 cm. above the center of the dial and actuated by an induction coil. The resistance of the flash-circuit was so great that the current did not jump across at the make of the primary circuit. There was therefore only one flash, namely, that caused by the break of the primary circuit, A condenser of sufficient capacity, in parallel with the break, prevented any disturbing sparking

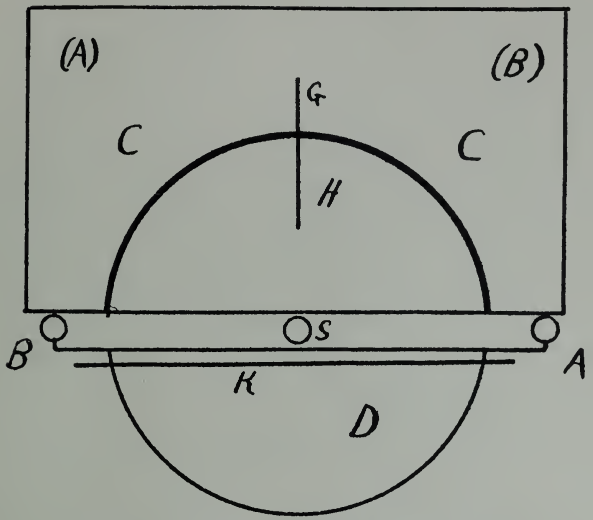

Fig. 1. Horizontal plan of disc and dial.

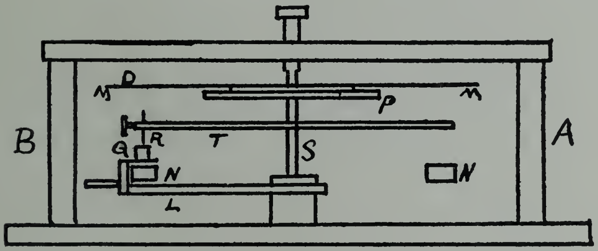

Fig. 2. Cross-section.

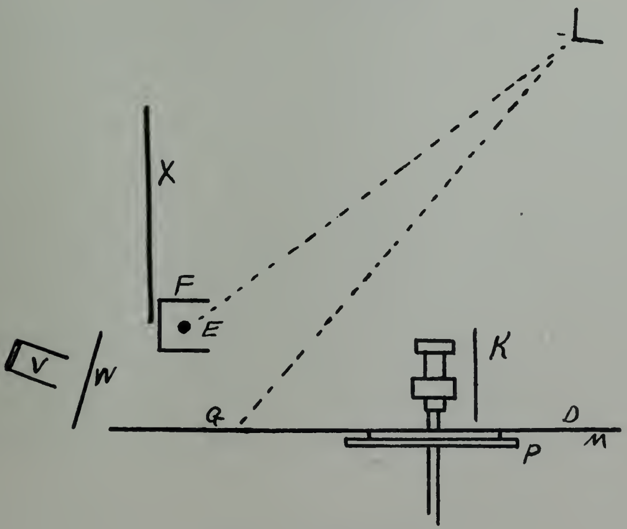

Fig. 3. Cross-section.

at this point. The flash in the Geissler tube was entirely inaudible to the subject. The tube was suspended in an oblong card-board box *F* (20 × 7.5 × 7.5 cm.), the anterior side of which was wanting. The interior surface of the box was black. The box was used in order to secure a more vivid flash, by intercepting the background of light which illumined the field of operation, and also to prevent the light from falling upon the dial and disc; which would have made an instantaneous photograph on the retina of the position of the index-hand. A screen *X* of black card-board above the box containing the Geissler tube shielded the eyes of the observer from the incandescent lamp *V* serving as constant source of illumination, The light of the latter was diffused by means of a plate of ground glass *W* placed vertically in front of it.

The machine was rotated by a belt transmission from an electric-motor which gathered impetus very quickly and maintained speeds of great uniformity, which were accurately controllable by means of a rheostat. The four rates I selected were one revolution in 2.25, 1.75, 1.25, and 0.75 seconds. The rates were obtained by observing the second-hand of a watch, until, through appropriate adjustment of the strength of the current in the rheostat there were four flashes (or clicks) in 9, 7, 5, and 3 seconds, according to the rate employed, the counting being continued in any one observation over five or six times these intervals. A key in the primary circuit enabled the experimenter to omit the flash or click at will.

The table upon which the time-machine rested was of such a height that the white disc was about upon a level with the eyes of the observer when seated. A screen *K*, 6 cm. in height and of the length of the diameter of the disc, placed just back of the center of the latter, intercepted the observer's view in the intervals of experiment. The long axis of the machine extending between the two vertical brass pillars was at right angles to the direction in which the observer faced, hence the finger lever could be moved in a clear sweep from pillar to pillar or through an arc of about 150°. For convenience of designation, the side of the circular brass dial at the right of the observer was called the 'A' side, and so denoted by the letter *A* placed at the right-hand end of the path, and correspondingly the side of the brass dial at the observer's left hand was called the 'B' side and marked accordingly. Correspondingly for the experimenter who stood opposite the observer the quadrants on the white card-board scale on his right and left were labeled the 'A' and 'B' quadrants respectively.

The method of procedure was as follows: the room was darkened except for the incandescent lamp serving to illumine the dial and the disc over its anterior half. The experimenter called 'fix "A"' (or 'fix "B"'), whereupon the observer moved the finger-lever to the *A* (or *B*) side, in the neighborhood of the right (or left) column, and rotated the contact-arm *T* until, in the case of the auditory stimulus, the metallic strip attached to the arm made contact with the center of the surface of the metallic block, attached to the finger-lever; or, in the case of the visual stimulus, until the needle *R* stood at the middle of the substituted cup *Q*, (the circuit during this procedure being of course interrupted by the key as above described). The revolving arm being held in this position, the experimenter shifted the card-board disc until the index-hand was brought to the desired position, as indicated by the numbered scale-divisions in the quadrant of the dial diagonally opposite to the position of the lever—in the 'A' quadrant if the lever is at the 'A' side, and in the 'B' quadrant if the lever is at the 'B' side. The experimenter then said 'Ready,' at which signal the observer released the contact-arm, and the experimenter closed the switch that started the motor, setting the machine in motion. When a uniform speed had been attained, for which three or four revolutions of the disc sufficed, the experimenter said 'now' and closed the key controlling the discrete stimulus. At 'now' the subject rose and with hand on finger-lever, fixating the goal, observed (by indirect vision) whether the disparate impression occurred synchronously with the passage of the index-hand, at a position within the 'A' quadrant, or at a position in the 'B' quadrant, *i. e.*, whether the complicating stimulus occurred at a position in front of the goal, relative to the direction of rotation of the disc, as in the case of 'A' settings, or at a position behind the goal as in the case of 'B' settings. Then, still steadilv fixating the central or zero position indicated by the goal, he moved the finger-lever and hence the position of the discrete stimulus in the path of the index-hand until, by advancing or retracting it, as necessary, the stimulus appeared simultaneous with the alignment of the index-hand and the goal. When satisfied that the object had been attained, the observer so indicated, and took his seat, whereupon the experimenter opened the switch controlling the stimulus-circuit and the switch controlling the motor. At the word 'set' the observer without moving the finger-lever rotated the arm until contact was made as before, enabling the experimenter to take the reading by the index-hand which was thus set at the position of judgment. The position being recorded, the procedure was repeated as before.

The technique specifically adopted called for the alternate shifting of the finger-lever, and hence of the stimulus, from 'A' to 'B.' Beginning each experiment-period with 'A,' six successive judgments were made with each rate, that is three upon the 'A' side and three upon the 'B' side, and with a uniformity in the order of the positions at which the preliminary setting was made in each trial. From side to side the positions were (in degrees)

A	B
60°	45°
75°	60°
45°	75°

Thus was obtained a perfectly definite order of starting points, which yet seemed to the subject hap-hazard as regards their distance from the goal. The above order was selected with a view to securing the utmost possible balance between the two sides.

Further, this provision for irregularly alternating succession of positions upon the 'A' and 'B' sides was made, along with the instruction to shift the lever 'carelessly' *i. e.*, avoiding returning it always to exactly the same position on either side, as a precaution against prejudicing the observer's results through his discovering any rigid method of setting out, and so inspiring in him an anticipatory feeling of the length of arc to be traversed by the discrete stimulus in order to bring it to the goal simultaneously with the index-hand.

With regard to the designation of the direction of error I follow precedent of Wundt and others, by designating as positive those errors in which the assumed position is later than the real position; and as negative, the errors in which the assumed position is earlier than the real one. Wundt read therefore from actual to assumed position in every case. Accordingly if the index-position which the subject judged to be at zero was really in the 'A' quadrant, as shown in the reading from the 'set' position, the error was positive; if the index-position which the subject judged to be at zero was really in the 'B' quadrant, the error was negative.

With regard to the order of the succession of the velocities: this was taken in three ways. In the first I began with the fastest rate and proceeded to the slowest; in the second I proceeded in the reverse direction, beginning with the slowest and gradually going over to the fastest; and thirdly the succession of rates was arranged in such a way that the rates intermediate in speed were made the extremes in the order of succession, and *vice versa*, so that the series was now 1.25, 0.75, 2.25, and 1.75.

With the light stimulus it was necessary to make a correction for the lag of the stimulus, due to the drag of the mercury in the cup, *plus* the latency of the spark-coil and Geissler tube, which together amounted, with the different rates, from fastest to slowest, to 2σ, 6σ, 8σ, and 14σ, respectively, to be subtracted from the results. This correction was obtained by turning out the light which normally illumined the dial, and inverting the box containing the Geissler tube, so that the tube was hidden from the eye, and the flash illuminated the dial and the index-hand. The index-hand was set by the usual method to read zero at the moment of the flash, and then put in motion at the speed for which the error was to be determined. In this way a photographic image of the actual position of the index-hand at the moment of the flash was produced on the retina and the deviation from the zero point could be readily noted. Black dots placed upon the scale-divisions facilitated the reading of the actual position due to the lag. The lag of the Geissler tube and spark-coil was independently determined by physical methods and found to be never greater than 1σ. The greater part of the lag was therefore due to the mercury-cup itself. No method is available for determining the latency of the physiological process in the retina.

Likewise to allow for the lag of the hammer employed for the sound stimulus, it was necessary to subtract 6σ, 5σ, 3σ and 2σ from the results obtained with the rates from fastest to slowest, respectively. These lags were accurately determined from kymographic records of the movement of the hammer and a tuning-fork curve, together with the time elapsing at each rate between the first making of contact and the instant of reaching the central point on the contact-block at which the readings were taken throughout the experiment. Further, account was taken of the time required for the sound from the hammer to reach the ear. As the hammer was approximately three feet from the subject's ear, this was assumed to be 3σ.

The corrected results of the first portion of my experimental work are given in part in Tables 1 and 2. Table 1 gives the average positions, *i. e.*, the averages of the displacements of the judged positions for each individual subject and rate for the sides 'A' and 'B' and for 'A' and 'B' taken together. The first column under 'subject' gives the five subjects by number. The second column 'rate' gives the seconds per rotation of the rates to which the figures correspond. The third column 'A' gives the average displacements when moving from the 'A' side, *i. e.*, when the flash was, at the beginning of the single experiment, much too early. The fourth column 'B' gives the average displacements, when moving the flash from the reverse or 'B' side. The fifth column 'total' gives the averages of the figures in the 'A' and 'B' columns. The sixth, seventh, and eighth columns give for the experiments with sound the averages corresponding to those in the third, fourth, and fifth columns for light.

Table 2 gives the average variations corresponding to the averages in the 'A' and 'B' columns of Table 1. No average variations are given corresponding to the 'total' columns in Table 1, because the only significant figures for such purpose would be the average of the variations for 'A' and 'B', the 'A' and 'B' averages in Table 1 forming distinct groups with variations about independent centers of equilibrium, as will be more fully brought out. If the average variation from the total were taken in the ordinary way, it would simply be adding the variable error to the constant error which is an entirely

TABLE I.

Subj.	Rate	LIGHT.			SOUND.		
		A	B	Total	A	B	Total
I.	0.75	+ 0.4	−21.0	−10.3	−23.3	−22.9	−23.1
	1.25	−14.0	−41.2	−27.6	−46.0	−51.2	−48.6
	1.75	−10.4	−49.3	−29.8	−36.2	−45.1	−40.6
	2.25	− 4.7	−40.0	−22.3	−23.8	−38.4	−31.1
II.	0.75	+ 9.4	+22.6	+16.0	− 0.2	+15.1	+ 7.4
	1.25	− 2.0	+ 2.6	+ 0.3	− 2.2	+ 3.6	+ 0.7
	1.75	−23.3	−18.5	−20.9	+ 3.4	+ 9.1	+ 6.2
	2.25	−18.1	−10.9	−14.5	+ 3.2	+11.5	+ 7.3
III.	0.75	+16.0	−17.9	− 0.9	+22.0	+12.3	+17.1
	1.25	+26.9	+ 0.3	+13.6	+32.5	+27.9	+30.2
	1.75	+24.4	+ 0.9	+12.6	+37.5	+37.5	+37.5
	2.25	+16.2	− 6.8	+ 4.7	+33.4	+33.4	+33.4
IV.	0.75	− 5.1	+ 0.4	− 2.3	−33.7	−40.3	−37.0
	1.25	+ 3.8	− 7.1	− 1.6	−28.7	−41.4	−35.0
	1.75	+ 4.1	+ 9.0	+ 6.5	−30.3	−29.5	−29.9
	2.25	+ 2.6	+ 0.5	+ 1.5	−35.3	−32.2	−33.7
V.	0.75	−31.4	−37.7	−34.5	−15.3	−24.0	−19.6
	1.25	−44.7	−62.6	−53.6	−45.4	−61.0	−53.2
	1.75	−28.2	−25.0	−26.7	−53.2	−37.8	−45.5
	2.25	−40.0	−43.1	−41.5	−68.6	−49.9	−59.2

unwarranted proceeding. The variations are given in their absolute values, not relative to the average displacement, because the measure of the average displacement is simply a convenient way of denoting a position on an arc of a circle by its distance from an arbitrarily fixed point. Other things being equal, we should have as much reason to expect a variable error of a given magnitude at one point on the arc as at another. It is obvious that if the average variation were expressed in percentages or in some such way in relation to the magnitude of the displacement, a displacement of zero (which might readily occur, as seen by the value of 0.4 in two cases) would

TABLE 2.

Average Variations.

Subj.	Rates	LIGHT		SOUND	
		A	B	A	B
I.	0.75	28.4	21.4	16.9	19.5
	1.25	23.1	22.4	10.9	16.7
	1.75	23.4	16.2	10.5	13.7
	2.25	16.6	19.7	14.5	17.7
II.	0.75	16.9	16.9	10.0	12.8
	1.25	12.7	15.0	8.9	8.6
	1.75	7.2	15.3	7.2	8.1
	2.25	19.7	13.5	8.3	7.2
III.	0.75	15.6	21.4	19.0	12.8
	1.25	18.5	15.6	12.1	17.9
	1.75	20.2	16.2	11.3	17.0
	2.25	15.6	25.0	13.5	17.7
IV.	0.75	16.9	16.9	8.6	14.2
	1.25	13.3	16.1	9.2	11.5
	1.75	20.2	13.7	6.4	10.5
	2.25	15.6	17.7	10.4	8.3
V.	0.75	27.3	31.8	22.5	23.2
	1.25	25.4	27.1	19.0	31.8
	1.75	32.4	33.2	18.6	33.2
	2.25	30.2	27.8	26.0	29.1

necessarily make the number representing the variable error infinitely large, which furnishes the *reductio ad absurdum* for the proportional statement of variable error and constant error. All values are expressed in σ throughout. Other experimenters fail to include in their results a table of average variations. The omissions are unfortunate, for a comparison of the average variations of my subjects with those of the subjects of the other experimenters would be interesting.

Five persons served as subjects for my investigation: Prof. G. M. Stratton, Dr. Knight Dunlap, Dr. W. D. Furry, Mr. T. A. Lewis, and myself. The experiments here reported were conducted for a period of two years, from the spring of 1907 to the spring of 1909.

My tables are constructed on the basis of the averages of 72 judgments for each subject, with each of the four rates employed, of which 36 are judgments made in the 'A' quadrant and 36 made in the 'B' quadrant.

First to be remarked is the fact that displacements, both positive and negative, occur when a momentary *light* impression is substituted for the discrete auditory stimulus hitherto employed in the complication experiment. In general the displacement with the visual stimulus is numerically less than with the auditory. The only marked exception is the case of Subject II, whose errors for each rate but one are greater with the light than with the sound impression. It is further seen that in general the direction of the displacement is the same for a given subject with both forms of stimuli, the exception being Subject IV, whose errors are all negative with the sound-stimulus, while with the light-stimulus his positive errors predominate. It is noteworthy that for each subject, with the sound, the direction of the displacement is consistently the same for each of the eight groups of judgments, (the exception appearing in the case of Subject II, whose displacement is negative in direction for the 0.75 and 1.25 sec. rates in the 'A' quadrant, all the other averages being positive) while for light there is among the groups corresponding to different rates considerable discrepancy as to the quality of the error.

With regard to the effect of the rate of rotation of the index-hand, and consequent rapidity of the succession of the discrete impressions: the relative displacement for the four rates employed differs with different subjects in amount and direction. For example, with Subjects I and II, the tendency is toward a more negative displacement with the slower rates, which accords with the results of Wundt and the other German investigators, while on the other hand, in the case of Subjects III and IV, a directly opposite tendency is shown, the lower rates producing a more positive result. With Subject V, no uniformity is to be traced in this respect. Further, the relative magnitude of the displacement for different rates, disregarding now the sign, varies with different subjects. For example, with the light-stimulus, the magnitude of the errors of Subject I are least with the 0.75 sec. rate, while the least error with Subject IV is at the 2.25 sec. rate, and for Subject V, at the 1.75 rate.

Considering now the results of 'A' and 'B', a comparison of the error obtained in each pair in the case of Subjects I, III, and V shows that the tendency of judgments, made when the position of starting out is in the 'A' quadrant, is positive in relation to 'B', that is to say that, in moving from 'A' or 'B', the tendency is to stop on the corresponding side of the average position of the two.

Comparing the errors of 'A' and 'B' in the case of Subject II, gives a result which is just the reverse of that given in the three cases cited. This discrepancy is only apparent however and the explanation is to be sought in the reversed procedure, which is characteristic of this subject's method in making a determination and in accordance with which the 'A' judgments and the 'B' judgments became virtually transposed. For in every judgment it was the practice of this subject to continue shifting the temporal position of the discrete stimulus from the starting position either before or after the alignment of index-hand and goal, until he found that he had overstepped the position of simultaneity, and hence was obliged to return to it. So that in setting out from the 'A' side, let us say, he invariably passed over to the 'B' side of the scale, until assured of a discrepancy, when he would finally set about making the discrete stimulus synchronous with the alignment—hence a 'B' judgment. In setting out from the 'B' side, his procedure was similar; the ultimate judgment becoming in reality an 'A' judgment. This explanation was not elicited from the subject after noticing the variance of his results. His manner of procedure had caught my attention from the first, without my seeing in it at the time any ulterior significance. Later, the subject himself, during the course of an experiment, voluntarily called attention to the fact that his procedure was invariably the reverse of the normal method above described. The subject was of course entirely in ignorance of his results when he made this statement. In the light of this explanation, the results which Subject II show with regard to the 'A' and 'B' direction of approach are entirely consistent with the results of the other subjects, so that the tendency of the 'A' judgment to stop on the 'A' or positive side of the average position of 'A' and 'B', and of the 'B' judgments to stop on the 'B' or negative side of the average position of the two is characteristic in general.

It may be noted that with Subjects I and III, the interval between the values of 'A' and 'B', that is, the distance of 'A' and 'B' from the average of both is much greater in the case of the Light impression than for Sound. For Subject II, the influence of the new position of setting-out (that is, reversing the direction after having crossed the goal) cannot but be off-set in a measure by the counter-effect of the influence of the original starting-point. Thus the effects of the two influences are composed in the results for Subject II, and probably account for the small separation of the 'A' and 'B' averages in his case. Subject IV presents results also less regular in this regard, particularly where the stimulus used was the light, but the explanation turns upon the same point as in the case of II, it having been also (though not invariably) the practice of this subject to over-reach the goal and then return to it.

An important factor which my experiments bring to light is the influence of the position of setting out upon the direction and magnitude of the final judgment. The effect is specifically shown by a comparison of the different results obtained, when setting out from different determined positions upon either side of the goal. To test this influence I compared for each subject the averages of the twelve judgments, obtained with each rate with the different positions of origin; that is, when starting out from the position of 75°, 60°, and 45° on the 'A' side and when starting out from the corresponding positions on the 'B' side. This comparison was made in the case of both light and sound stimuli. The results are given in Tables 3 and 4, for light and sound respectively. The averages are here given in degrees. The tables show the

TABLE 3.

Light.

STARTING POSITIONS. IN DEGREES.

Subj.	Rates.	A.			B.		
		75°	60°	45°	75°	60°	45°
I.	0.75	+15.80	+ 3.10	−12.30	−18.4	−12.10	+ 1.5
	1.25	− 1.7	− 3.8	−13.1	−17.0	−11.9	− 3.9
	1.75	+ 1.7	+ 2.0	− 6.5	−11.3	− 6.4	− 5.0
	2.25	+ 2.5	+ 3.6	+ 1.4	− 5.4	− 3.8	− 2.1
II.	0.75	+11.83	+ 0.66	− 2.08	+ 8.58	+12.75	+14.08
	1.25	+ 0.33	+ 1.5	− 1.25	+ 5.0	+ 1.25	+ 0.16
	1.75	− 3.25	− 4.25	− 3.75	− 3.83	− 0.83	− 1.5
	2.25	− 0.41	+ 0.16	− 2.66	+ 0.25	− 0.83	+ 1.0
III.	0.75	+18.25	+10.66	+ 0.25	−18.08	− 8.91	− 0.16
	1.25	+12.91	+10.33	+ 2.66	− 1.5	+ 1.75	+ 5.48
	1.75	+11.5	+ 6.0	+ 1.75	− 0.75	+ 1.91	+ 5.16
	2.25	+ 5.58	+ 6.5	+ 2.91	− 1.34	+ 2.5	+ 2.16
IV.	0.75	+ 3.0	+ 2.16	− 6.25	− 7.33	− 0.33	+ 7.33
	1.25	+ 4.08	− 0.48	+ 3.91	− 0.58	+ 2.0	+ 0.58
	1.75	+ 4.83	+ 2.5	− 0.16	+ 2.91	+ 3.58	+ 5.5
	2.25	+ 4.5	+ 1.75	+ 0.91	+ 2.25	+ 4.75	+ 1.58
V.	0.75	− 1.33	−16.33	−28.25	−31.5	−23.66	− 5.0
	1.25	− 6.66	−12.25	−19.41	−22.16	−18.91	− 7.41
	1.75	+ 0.16	− 4.33	−10.25	− 7.41	− 4.5	+ 1.91
	2.25	− 2.33	− 3.16	− 6.58	− 6.66	− 8.0	+ 0.66

TABLE 4.

Sound.

STARTING POSITION. IN DEGREES.

Subj.	Rates.	A.			B.		
		75°	60°	45°	75°	60°	45°
I.	0.75	− 1.91	− 8.0	− 8.75	−18.41	− 7.08	− 2.83
	1.25	−11.08	−14.33	−12.25	−15.91	−12.41	−10.00
	1.75	− 5.08	− 5.16	− 7.83	− 9.91	− 7.41	− 7.75
	2.25	− 2.66	− 2.75	− 4.25	− 8.5	− 5.41	− 4.25
II.	0.75	+ 5.58	+ 5.41	+ 1.0	+ 5.5	+10.16	+18.25
	1.25	+ 3.33	+ 0.5	+ 1.33	+ 0.75	+ 3.83	+ 5.08
	1.75	+ 3.08	+ 1.16	+ 0.83	+ 1.91	+ 3.25	+ 4.25
	2.25	+ 3.16	+ 0.16	+ 0.33	+ 1.08	+ 2.66	+ 3.75
III.	0.75	+21.25	+15.25	+ 6.0	+ 2.83	+12.08	+14.66
	1.25	+14.41	+12.16	+10.25	+ 4.91	+11.0	+16.75
	1.75	+11.33	+ 9.0	+ 7.41	+ 6.08	+10.5	+12.5
	2.25	+ 8.83	+ 5.08	+ 5.5	+ 3.58	+ 7.0	+ 8.91
IV.	0.75	−13.66	−13.91	−16.5	−23.58	−17.08	−12.58
	1.25	− 6.41	− 5.41	−10.91	−10.66	−10.91	− 8.58
	1.75	− 6.0	− 5.33	− 6.83	− 7.33	− 7.0	− 7.08
	2.25	− 4.91	− 6.16	− 4.91	− 6.58	− 3.83	− 5.91
V.	0.75	+ 0.75	− 2.66	−19.83	−18.58	−10.5	− 2.08
	1.25	− 9.33	−10.83	−18.75	−28.0	−11.75	− 6.66
	1.75	− 9.41	− 7.0	−14.5	−12.25	− 8.33	− 1.08
	2.25	−10.16	− 9.0	−12.5	− 9.83	− 7.41	− 4.08

remarkable regularity of the influence of the starting point in the coincident progression of final judgments and original positions, the final judgments of the 'B' side becoming progressively more positive as the starting point is shifted in a positive direction and correspondingly the final judgments of the 'A' side assuming a more negative position parallel with the more negative shift of the initial setting of the index-hand.

Because of the resulting feebler contact as the rates became progressively faster, the intensity of the stimulus diminished with increasing rates. As this factor introduced the question whether the varying results obtained with different rates were due to the corresponding variation in the intensity of the stimulus, new experiments were undertaken with a view to studying the influence of the intensity of the discrete stimulus. Using Subjects I, II, III and V, I obtained with each on three different days, eighteen judgments with a weak intensity, and the same number with a stronger intensity, the eighteen judgments consisting in both cases of nine of 'A' and nine of 'B.' Sound was used throughout this variation of my experiment. I proceeded by giving groups of six trials, that is, three each of 'A' and 'B,' and alternating the intensity with successive groups. The two intensities were designated 'Weak' and 'Strong,' the weak being the weakest stroke employed in the experiments, and the strong consisting of a very loud, sharp stroke. The results as shown in Table 5 were practically negative, the only considerable difference occurring in the averages for Subject V, whose determinations were always erratic. The values in Table 5 are given in σ, and are the excess of the average errors with 'Strong' stimulus over the average error with 'Weak.'

TABLE 5.

Intensity. Sound. Rate 1.25.

Strong *minus* Weak.

Subj.	A.	B.	Total.
I.	− 3.47	+ 3.47	0.0
II.	+ 0.01	+ 3.47	+1.74
III.	0.0	0.0	0.0
V.	−10.41	+17.35	+3.47

Since along with Wundt's denial of the possibility of obtaining a displacement in cases in which two momentary stimuli are of the same sense, he excepts the cases in which the conditions make possible the wide separation of the two stimuli, as upon the upper extremity and the lower extremity, or with one and another of a pair of organs of the same sense, as with first one eye and then the other; a series of experiments were now made in which the two visual impressions, the flash and the passing of the index-hand, fell upon contiguous areas of the retina. The reader will recall that while, in the first experiments, the impressions were on adjacent portions of the retina, they were not strictly contiguous, the Geissler tube being separated from the goal (and its neighboring regions) by an interval of approximately 12.5 cms. The condition of actual contiguity was satisfied by inverting the box containing the Geissler tube, so that the flash fell upon the dial, while the subject did not see the tube at all; the margin of the area illuminated by the flash coinciding with the edge of the dial and hence of the scale, along which moved the index-hand. The flash was limited to the dial in this manner, and not permitted to fall upon the index-hand, for in such a case a retinal photographic image of the actual place of the index-hand would have been produced, thus precluding the conditions essential to the conduct of the experiment. The results of this experiment are given in Table 6. Each number given under 'A' and 'B' in this table expresses in σ the average of twenty-four judgments. As the results show, where conditions are introduced which cause contiguous visual images to fall upon the retina, a time-displacement occurs not differing in direction from the time-displacement observed where the stimuli were allowed to fall upon nerve-terminals of the retina which were more remote from each other.

TABLE 6.

Contiguous Visual Stimuli. Rate 1.25.

Subj.	A.	B.	Total.
I.	−10.05	− 42.5	−26.1
III.	+49.04	+ 23.0	+36.02
IV.	− 9.85	− 32.6	−21.22
V.	− 7.45	−103.05	−55.25

In the case of Subject I, the magnitude of displacement is about the same in the two cases. In the cases of III and IV the magnitude is greater with the contiguous stimulation; and in the case of V, although the magnitude of the total is about the same, 'A' and 'B' are farther apart.

To test the validity of the alternative requirement which Wundt makes as a condition of obtaining a temporal displacement in the case of two simultaneous impressions belonging to the same sense-realms, namely, that they occur each upon one of a pair of organs of the same sense, I conducted a few experiments in which the subject observed with monocular vision.

Performing a series of experiments upon different days on Subjects I and IV, and having them alternately use the right and left eye, I again obtained results, agreeing essentially with the readings obtained when the subjects presumably employed binocular vision, but showing a more exaggerated displacement. If, however, it be suggested that even when both eyes were open, the subjects really employed monocular vision, the results are in no wise affected.

The general method of my experiments, *i. e*, the active approximation, by the subject, of the discrete stimulus to a fixed position of the index-hand, introduces, as has been clearly brought out above, factors which are not explicitly present in the judgments obtained by the older methods, although it is by no means certain that they are not operative in those cases. In order to find whether the general averages, in which these factors may be supposed to compensatefor each other, differ much from results obtained by the older methods, further experiments were undertaken with Subjects I, III, and V, in accordance with the Wundt procedure. For this purpose another scale was made which did not bear the black strip at the zero point to indicate the goal, but bore instead the scale drawn in black lines 13.5 mm. long, and 1 mm. wide, five degrees apart, and extending 75° in both directions from the central point. For accuracy in recording, these were numbered in pencil, but so faintly as to be indiscernible to the subject. Following the classic methods the experimenter placed the discrete stimulus at a position of the index-hand unknown to the observer, and when the latter had declared the position at which the index-hand seemed to be, at the moment of the discrete stimulus, the two positions were compared and the difference between them, *i. e.*, the magnitude of the displacement, if any, recorded; the quality of the displacement as usual being called positive if assumed position comes *after* the real one, and negative if the apparent position comes *before* the actual position.

In these tests, the series used, in the experimental hour, consisted of nineteen different 'settings' of which nine were at different positions in the 'A' quadrant and nine at different positions in the 'B' quadrant, the other position being at zero. Only one rate was used, the 1.25. The discrete stimulus was necessarily sound. There was but one series given in the case of Subject I, and for Subjects III and V, two series each were obtained. The results are given in σ in Table 7, each number under (A) and (B) being the average of nine determinations, namely, the nine given in the quadrants designated as A and B respectively in the previous experiments.

TABLE 7.

Passive Method. Rate 1.25.

Subj.	(A)	(B)	Av.
I.	+20.82	− 41.64	−10.82
III. (1)	+34.7	− 27.76	+ 3.47
(2)	+38.17	− 48.58	− 5.20
V. (1)	−45.11	− 90.22	−67.67
(2)	−65.93	−100.63	−83.28

For Subject I, the displacements for the settings in the 'A' quadrant were all positive, and in the 'B' quadrant all were negative. The same was true for Subject III, with three exceptions in the 'A' quadrant, although the average was there positive. Subject V is so far in agreement with the other two that the displacements are more negative for the B quadrant.

III. DISCUSSION AND INTERPRETATION.

The experiments which I conducted after the method of Wundt (see Table 7) brought out a result strikingly different from what was found by Wundt and Geiger. This result was the displacement of the apparent simultaneity in opposite directions on the two halves of the dial, the errors being almost all positive in the 'A' quadrant and negative in the 'B' quadrant. It is true that the German experimenters had found a difference in this direction between the results for the two halves of the dial, explained by them as due to eye-movements; but the difference they found was merely a greater positivity (or less negativity) on the one side as compared with the other, and not a complete reversal with considerable magnitudes.

Now this opposition between the results given in the 'A' and 'B' quadrants is obviously the outcome of some feature of my experiment, which is not present in the experiments of Wundt and others. It will be remembered that in this part of my work the dial presented to the subject was uniformly scaled throughout, there being no conspicuous mark to indicate the middle point of the dial, as was the case in my other experiments.

This difference between the errors in the two quadrants agrees in kind with the difference between the errors when moving from 'A,' and the errors when moving from 'B' in the preceding experiments on the same observers, although in these preceding experiments the factor of eye-movement is either excluded or at least reduced to minimal terms. It is therefore impossible to escape the conclusion that the factor which produced the results in question in my experiments after the Wundtian method was carried over from the preceding experiments; for, furthermore, the conditions of my last experiments differed from those of Wundt and Geiger essentially in being preceded by these other experiments by a different method.

The factor in question probably consists in the greater attention to the middle point of the scale, resulting from the preceding experiments in which the middle point indicated by the black strip ('goal'), was intentionally made the focus of interest. It is impossible to suppose that the habit of fixing the attention upon the middle position, to the nurture of which the usage of the preceding experiments with these subjects contributed, is not unconsciously carried over into the subsequent experiments, with the effect of influencing the judgments in a manner similar to that in which the preceding judgments were influenced.

Geiger noted a factor analogous to the one in question, which consisted in the fortuitous effect upon the attention of the uppermost and lowermost positions of the scale. This factor first appeared in the experiments in which the telescope was interposed, thus introducing a division of the dial into quadrants by the cross-threads of the instrument. But as Geiger argues, an influence analogous to this actual partitioning of the dial by means of the telescopic field, is doubtless operative, though of course in a less degree, in experiments of the original form. For there too, the top and the bottom at least of the dial become, as it were, critical points in the path of the index-hand. The very fact of these points being uppermost and lowermost gives to them of itself a feature of distinction. Further, these points represent for us, as they do in all symmetrical objects, median positions. These points possess then of themselves a certain specific interest, analogous to the interest we find in general in the median region of all bilaterally symmetrical objects, as especially exemplified in the human body.

So much for the influence of the 'Umkehrpunkt,' as brought out by Geiger, upon determinations made with the complication-clock. What of its influence upon determinations made in experiments in which the complication-pendulum has been the apparatus employed? From an analysis of the different elements entering into the conditions of experimentation with the pendulum apparatus, we may justly infer that the effect of a critical point is still more potent in the use of this machine, because of the oscillation of the index-hand about a middle point. For in this form of apparatus we have in addition to the effect upon the attention of a middle point of the scale, as cited by Geiger, likewise the influence due to the fact that here the middle point on the scale is also the middle point as regards the path over which the index-hand swings. This latter factor has the effect of emphasizing the middle position in a two-fold manner, that is, in regard to space and in regard to time; for the length of vibration, measured in degrees, of the index-hand upon one side of the mid-position is exactly equal to the length of its vibration upon the other, and the time of vibration of the index-hand, upon one side of the central point is exactly equal to the time of vibration upon the other. Hence it is the central point, temporally as well as spatially considered. Finally, a further factor tending to give preëminence to this middle position lies in the fact that this position is the only one throughout the entire excursion of the index-hand at which the velocity is constant. Toward this point and only as far as up to this point, the index-hand is increasing in velocity; leaving this point and immediately after passing it the velocity is decreasing, so that independently of the influence upon the error of the factor of acceleration and retardation *per se* (if any such influence exist), this factor tends indirectly to emphasize in a special manner the position at which the index-hand is at its center of oscillation; and thus is added a third element to the general influence serving to emphasize the attraction of the middle or uppermost point upon the scale. Of course this last element, as well as the other elements contributing to the total influence tending to render prominent the critical, turning-point in the path of the index-hand, is throughout unconsciously operative. The question that here suggests itself is whether, after all, the difference in the results on the two sides of the dial, where the pendulum machine was used, was really due to the factor of acceleration and retardation, or rather to this factor of the difference in the influence of the central point in the scale, according as the position of the index-hand was on the one or the other side of it when the bell-stroke or other discrete stimulus occurred.

Naturally the consideration of this factor calls into question the results of the work of Klemm, which, as above stated, was planned with a view to isolating the influence of the factor of eye-movements, according as the judged position was upon the side of ascent of the index-hand or upon the side of descent, from the influence of the factor of acceleration and of retardaation occurring concomitantly under the same conditions. Was the factor which Klemm thought he isolated really the factor of acceleration and retardation, or was it the factor which our experiments have shown to be so considerable under the simpler conditions of experiment obtaining in the use of the complication-clock, and which, it may be justly inferred, would operate so much more effectually with the use of the pendulum apparatus—namely, the factor of the influence upon the attention of the chief or central point in the path of the index-hand?

We have, then, throughout all such experiments, the tendency to elevate in importance the uppermost or, in Klemm's work, the lowermost, region of the dial; but besides this unconscious influence, it is the deliberate intention of my principal experiments to concentrate the interest upon this region, and make of it the goal to be attained by the index-hand at the moment of the discrete impression.

Let us call this factor of the attraction of the predominant point of interest, which is seen to be especially operative in my experiments, factor No. I. Now, assuming this factor alone to be present, judgments from the 'A' positions, in the normal procedure, in which the subject moves the position of the stimulus directly toward alignment of the index-hand with the goal, would be invariably on the 'A' or positive side,[1] and conversely, judgments from the 'B' positions would be always on the 'B' or negative side. For the effect of the attraction of the objective point or goal is to cause a given position of the index-hand to seem nearer the goal than it really is, and accordingly an actual position, which is a certain distance from the objective point will seem to be at that point. Positions still nearer the goal than that which the subject first judges to be the correct one may also seem correct, as shown in the case of Subject II, who actually moved to such nearer positions and even to a position beyond apparent simultaneity.

Now since results by my original method for each subject in nearly every rate show the determinations, when setting out both from the 'A' side and the 'B' side, to have the same sign, *i. e.*, to be on the same side of the goal, it is evident that a second cause or group of causes is operative which is sufficient to overcome the influence of factor No. 1, on at least one side of the goal. Since factor No. 1 changes the direction of its influence when the goal is past, or at least since it becomes a permissive influence in the new direction, the two factors are now really coöperating, *i. e.*, factor No. 1 now probably operates to permit an effect of factor No. 2 greater than it would have if factor No. 1 were not present. Certainly it does not lessen it. For example, Subject I in Table 1, gives throughout (with the single exception of the 'A', 0.75 rate, for light) negative determinations; which means that the determinations both from positions of setting out in the 'A' quadrant and from those of the 'B' quadrant were in the 'B' quadrant. Hence, in obtaining the former determinations the goal must have been crossed, which means that in addition to factor No. 1, which allows the determination to stop before reaching the goal, there was present a further influence which operates to carry the judgment by the goal and over to the 'B' quadrant or negative side.

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[1] Speaking of course in terms of my experiment, where the judged position was finally zero in each case, and the record of the judgment, the distance of the real position from that judged position.

A glance at Table 1 shows that while the movements from both directions terminate in the same quadrant, they do not terminate even approximately at the same point. From this it is clear that the terminus of the judgment in so far as concerns the influences as yet considered, is not a point but a region within which any point might serve. The subject therefore who proceeds to the position of his final judgment as in the normal way, moves into this zone and stops, whereas the subject who inverts this procedure moves through this zone, and then back into it. We may say then that the difference between the values of 'A' and 'B' represents the width of this indifference zone. What is here called the indifference zone, corresponds to the region to which Klemm applied the name of 'Simultaneitätsbereich.' What the constant factor (No. 2) is, which tends to give the average of 'A' and 'B' a negative or a positive value as the case may be, is a question with which we shall later concern ourselves.

The next factor to be considered is the movement of the position of the discrete stimulus from the starting point towards the goal. This we may call factor No. 3. Whatever effect this factor may have, we should expect to be dependent upon the length of the movement, *i. e.*, whether starting out from a position at 75°, 60°, or 45° from the goal in one or the other quadrant. It is important to note that since the positions in question correspond on the two sides of the dial, and the movement is in opposite directions on the two sides, this influence must cancel on the averaging together of the 'A' and 'B' results.

Tables 3 and 4 show for the light-stimulus and the sound-stimulus respectively a pronounced effect of the different positions of setting-out on the two sides, the tendency being on the 'A' side to make the judgment more positive, when starting from a more positive position *i. e.*, from a position more remote from the goal; and conversely on the 'B' side, the judgments are more negative according as the position of commencing is more negative, *i. e.*, more remote from the goal. So that, in general, in commencing successively with positions of 75°, 60° and 45° on the 'A' side, the judgments become progressively less positive, while employing successively these same positions of starting out on the 'B' side, the resulting judgments are progressively less negative. This may be interpreted first, as an instance of the prevailing tendency to bring movements and even judgments to an average; or second, it may be regarded as due to an influence of the starting-position on factor No. 1 or on factor No. 2. The exact details of this second interpretation are not apparent while the first is probably true in any event. This influence of the starting-point whether expressed through the averaging of movements or other processes, or through the influence of the starting-point on the attention, and thus on factor No. 1 or No. 2, or on both, contributed largely to the variable error. If each group from a given starting-point is treated separately its variable error is low.

It was stated in the beginning of this section that the deviation in opposite directions of the judgments in the opposite ('A' and 'B') quadrants of the dial, as shown in the experiments I conducted after the manner of Wundt, might be ascribed to the contrary effects of eye-movement upward in one quadrant and downwards in the other, except that under the conditions of my method of experiment the factor of following eye-movements is practically *nil*. I wish now to show why, in consideration of my altered method of experiment, this hypothesis is not to be entertained. It was the experience of all my subjects that the eyes were kept constantly fixed on the goal. If any eye-movements did occur, they could have been only slight and therefore confined to the middle portion of the scale, in which region the index-hand moved in a nearly lateral direction. Hence, the eyes being fixated upon the central goal with approximate constancy, movements of the eyes in the way of following the index-hand could have been only in a direction practically lateral, not up and down. If, however, we should suppose that larger eye-movements do occur, the effect would be as follows: On the 'A' side, where the index-hand is ascending, the eye-movement, due to the attempt to follow the course of the index-hand, would bring about, as Geiger has shown, a negative tendency in the displacement, and on the 'B' side a positive tendency. Now since a position of the index-hand on the 'A' side always corresponds in my experiments to a positive displacement, and a position on the 'B' side to a negative, the effects of following eye-movements would be simply to diminish the measured error in all cases and not increase the error on one side and decrease it on the other, as in Geiger and Wundt's method. Thus in any case, the results by my 'goal' method do not involve any factor due to eye-movement, except in so far as the separation of the 'A' results and the 'B' results may *possibly* be thereby less than it would otherwise be.

I have referred above (p. 29) to the different orders of succession of the velocities employed, first the order in which the velocities were increased from rate to rate, then the reverse of this procedure and finally the arrangement of the series whereby the extremes of speed became intermediate in position and the intermediate rates occurred at the beginning and at the end of the experimental hour. It might be expected that this succession of rates one after another, particularly in the last-mentioned order in which the slowest rate is made to succeeed directly the fastest, would bring to light some influence of contrast. On careful scrutiny of the results, however, no essential difference appeared between the results for the same rates in the different orders. The method employed tended to eliminate contrast effects, because, determinations being made in groups of six with each rate, and requiring a number of revolutions for each judgment, the contrast effect, if present, would, unless very large, affect only the first judgments at a given rate, and hence would become inconsiderable in the average of the first judgment with the other five. The principal result of a contrast, if present, would be an increase in the mean variation.

Also, because of the order of succession of the four rates employed, the influence of practice does not prejudice the results of my experiments. By practice is here understood the effect of repetition and of adaptation to a constant condition. Since every rate was taken at each experimental hour, the averages given (see Table 1) each represent approximately the same amount of practice. However, the results for each rate were carefully gone over with respect to their time-order, and no variation of the results could be correlated with the progress of the experiment, *i. e.*, with the increasing repetitions.

In my discussion of Geiger's results an account was given of the two methods of observation which this experimenter found to be characteristic of the two types into which he differentiates his subjects, designating them the "naïve' and the 'reflecting' methods accordingly. I stated that Geiger attributed no exact significance to the terms by which he denoted these opposed types, but that he believed the two methods of observation thus designated often merged more or less into one another, so that the distinction which these terms indicated, was by no means a rigid one. Further, Geiger states that the terms are in themselves inadequate to classify the types in question; that, while the subject denoted as 'naïve' has in his general method of determination a tendency more or less aptly described by this term, it is yet unsatisfactory in its import, and he finds the term 'reflecting,' by which he designates the subject accustomed to the other habit of observation, equally inadequate. It is apparent that the terms are on the whole but faintly descriptive of the subjects' methods, and Geiger is frank to own their ineptitude. He therefore offers as alternative to this terminology the terms 'scale-type' and 'index-hand-type' of judgment—the scale-type corresponding to the 'reflecting' and the index-hand-type of judgment corresponding to the 'naïve.' For, it will be remembered, it is characteristic of the 'reflecting' type that the observer notes quickly and holds fast to the *position on the scale* at which the index-hand arrived, at the moment of entrance of the discrete impression; on the other hand it is characteristic of the subject of the 'naïve' type that he follows the index-hand with more or less of nonchalance until, without effort on his part, its position at the moment of the discrete impression seems established for him, whereupon he notes the corresponding position on the scale.

Undoubtedly Geiger has the right clue in assuming two types of observers, or rather two types of judgment, but his failure to isolate them sharply from one another may mean that he has caught only the one aspect of them which appears in his particular conditions of experiment.

My experiments had not proceeded far, when it became apparent that the method of making determinations was not constant; not that certain subjects followed exclusively a certain method throughout, while others observed according to an entirely different method, but that it was early seen that each subject inclined now to one way of proceeding and now to another way, so that two factors came prominently into consideration according to the methods in question. These factors were in the first place the perception of the *spatial* position of the discrete stimulus in relation to the alignment; *i. e.*, the angle between the goal and the position of the index-hand when the discrete stimulus occurred and secondly the perception of the *temporal* relation between the two stimuli; that is, between the moment of the alignment and the moment of the discrete stimulus, or *vice versa*. We have then two types or methods of procedure according as the spatial or the temporal factor is the more prominent, and I have accordingly designated them the spatial and the temporal types.

It should be stated that the difference in the nature of the judgment depending upon whether the subject's determination of lack of simultaneity was really the estimate of the purely *temporal* interval between the discrete impression and the alignment, or of the spatial separation of the two corresponding positions of the index-hand, was brought to my attention entirely independently of Geiger's statement with regard to the two types of determination discriminated by him, and it did not occur to me at the time that what I called the spatial and temporal methods of determination could be correlated with the methods he described. That Geiger's 'naïve' and 'reflecting' types correspond to the two forms of judgment revealed in my experiments I hope to make clear in what follows. With Geiger, the 'naïve' type is the type of observer whose custom is to regard primarily the index-hand—to correlate the index-hand *per se* with the discrete stimulus. The observer of this type asks himslf *where* was the index-hand at the stroke of the sound; that is, he locates it not with regard primarily to the scale or marks on the dial, but rather by its angular position, probably by reference to the vertical and horizontal orientation. This does not mean that he explicitly recognizes the orientation or measures in degrees. But the spatial position of the hand at a given moment is noticed without the aid of the dial or scale.

On successive rotations of the index-hand, the observer continues to note the positions in the same way, and hence notices the angular distance between the position at which the index appears to be when the discrete stimulus occurs one time, and the position next time. When finally this deviation, which appears to him each time as the *error* in the preceding estimation, ceases to occur, the observer accepts the determination as final, and notes the scale position, if it have not previously obtruded itself upon him. It is seen that in this procedure the subject's estimate of the position of the points of his supposed error and of the consequent correction is expressed in spatial terms. The question of 'how far ahead?' or 'how far behind?' is the subject's new determination with regard to the preceding apparently correct position is a question of the measure of the angular interval between the positions.

This spatial element in the determination is especially brought out through the method involved in the use of apparatus such as I employed, in which the subject is called upon to perform the active placing of the temporal position of the discrete stimulus at the moment at which the index-hand shall pass a certain definite point on the dial, setting out from various positions more or less remote, upon either side of the position to be attained. It was found that in first setting out certain subjects more readily took account of the spatial than of the temporal interval between the index-position at which the discrete stimulus was given them (75°, 60°, 45° 'A' or 'B') and the position (at 0°) to which they had to shift it. The spatial separation was for them the easier datum to reckon with in this case. Where the interval was wide, as always in the beginning of each experiment, it was the more comfortable way of regarding the separation of the two positions. As the two termini stood out pictorially before him, the subject quite unconcernedly noted them together. As the method involved less the feeling of strain upon the attention than would have been occasioned through the attempt to connect up in a single conceptual act the temporal relation of the two impressions, at least for a certain type of subject, such a subject, upon setting out, fell naturally into this habit of observing. It is because of this characteristic leisure-attitude of the subject in making judgments according to the method which obtains in spatial estimates of the lack of simultaneity that I am led to identify the observer of this type with the one whom Geiger calls 'naïve' or of the 'index-hand' type.

In the second method of observation the time-element is predominant. With this type the influence of the sense of the spatial relation of the two positions: *i. e.*, the position which the index-hand first appears to occupy at the occurence of the discrete stimulus and the position of zero at which the subject is to place the latter, is apparently eliminated. The subject makes of it a time-judgment, and the experiment becomes substantially an attempt to reduce an initially large time-interval to zero.

In experiments like Geiger's the 'reflecting' subject watches a scale-division to notice if the appearance of the index-hand at it is simultaneous with the appearance of the complicating stimulus; and if it is not, he tries other scale divisions, to one side or the other of the first. From this description it is obvious that the temporal form of judgment as it appears in my experiments is justly to be correlated with the 'reflecting' or 'scale-division' type of judgment as shown by Geiger. Now the temporal method is quicker than the spatial; that is the subject reaches his final determination after fewer revolutions of the index-hand than are required for the subject whose judgments are of the spatial type. The reason is undoubtedly this: in the temporal form of judgment the subject is dealing successively with two sharply limited, clear-cut impressions; the momentary discrete impression and the momentary impression of the scale-division which the subject selects to note as the index-hand sweeps past it; each is brief and clearly defined. But in the method of spatial determination the subject is necessarily occupied with both details at once, and moreover, perceives the angular position of the index-hand indistinctly on account of its being in motion and traversing the very angle it marks off. Hence the individual judgments are less clear by this method, and more revolutions are needed before the subject arrives at a satisfactory determination, just as Geiger found to be the case with the 'naïve' type as compared with the 'reflecting'. In this agreement in difference of time required for making a judgment, there is then further ground for correlating my temporal and spatial types with the 'reflecting' and 'naïve' types respectively of Geiger.

It must not be understood that the two types which I have designated 'spatial' and 'temporal' are to be ascribed to individuals adhering rigidly to one or other of them: that one observes exclusively with the assistance of spatial data and another type exclusively with respect to temporal data, but rather that they are two methods which coöperate in most cases. It seems probable therefore that each subject is swayed more or less by the influences of both methods, and that the type can be determined only as the predominance of one of the two methods. As said above, it would seem that in my form of apparatus, the obviously wide spatial interval between the place of the stimulus when setting out and the goal to which it is to be brought renders the spatial method of observation the more convenient at the beginning of a determination. To compute the interval in terms of the spatial perception of its termini is instinctively felt to be the more economical procedure. When, however, the interval becomes narrow (as when the subject has shifted the stimulus toward the goal) and may not be readily computed because of its narrowness the tendency to rely upon the relative *temporal* position of the two stimuli (discrete impression and alignment of index-hand with central goal) doubtless comes more strongly to the fore. And further, precisely in consequence of the obliteration of the appreciable spatial interval—the interval over which the index-hand is required to move—the method becomes *ipso facto* the method of 'scale-division' observation, *i. e.*, in my terminology, the temporal method. The data are reduced to two: alignment of the index-hand with the scale-division or the goal, and the discrete stimulus,—flash or sound—which is to be correlated with it.

Of course it is not impossible that the subject of the temporal type should sometimes adhere to this method even in the beginning of an experiment, feeling the temporal relation of the stimulus to the goal when first setting out; but even in the temporal type of subject it is impossible to say how great a part in general the beginning spatial tendency plays in the ultimate determinations.

The two factors of spatial and temporal judgment were introspectively observed by Subjects I and IV, who were convinced (Subject IV was myself) of the validity and importance of the distinction. Experiments were later undertaken with Subjects I and IV, in which the attempt was made to isolate these two factors by having the subject observe at one time the spatial, at another the temporal relation of the stimuli but these experiments were abandoned because of the great difficulty of adhering consistently throughout to either of the two methods. It was not possible to isolate them adequately, and the devising of methods by which this isolation shall be suitably made belongs to another and lengthy experiment.

It is sufficiently clear, however, that the two factors are present, and my own attempts at an analysis of the elements involved lead me to the conviction that their importance demands a new development of methods and apparatus for adequate experiment in the future upon this problem. Here, I purpose to show that the phenomena of positive and negative displacements depends upon these two types of judgment, and that the heretofore adduced theories are correspondingly inadequate.

Geiger says "The results of the complication experiment appear to contradict normal reason:" for "one would naturally expect that the sound would always be united with a later scale-division than that with which it is actually simultaneous" and the explanation that follows assumes the onus of allaying our disappointment in this expectation. But Geiger neglects one important consideration. Were the assumption true that we are here dealing with a pure time-judgment, then it would undoubtedly contradict the presuppositions of normal experience to record that the subject, instead of selecting a later position than the actual one, should in general, actually select a position on the scale earlier than the real one. But we are not here dealing with a pure time-relation; in other words, the complication experiment has not been, in truth, the problem which all this while it has been tacitly assumed to be, namely, the determination of the *temporal* relation of a discrete impression to a series of similar impressions, into the course of which it enters at regular intervals, but the experiment is seen to become, on analysis, a determination of the *spatial* as well as of the temporal relation of the stimuli in question. Doubtless this prevailing misconception of the essential conditions determining the judgment in complication work is responsible for the wide disparity in the explanations which different observers offer to account for the phenomena, and its recognition reconciles in a measure the fundamental variances amongst them.

Let us come now to the explanation of the displacements, based on the difference in the types of observers, which my method of experiment brings out. In agreement with Geiger and the other German experimenters, I regard the positive errors, *i. e.*, the displacements to a position later than the actual one, to be the more natural errors. On the other hand, in opposition to these theorizers, I hold that these errors would actually be found prevailing were the displacements in reality only time-displacements; *i. e.*, were the measured error an error exclusively in estimation of the temporal relations of the stimuli in question; were the problem simply one of time-relation as psychologists have heretofore implicitly assumed it to be. For even in the experiments which do not exclude the presence of the spatial factor, the positive displacements are the prevailing ones with subjects who are predominantly of the temporal type. For the subject who observes strictly the temporal relation of the stimuli the discrete stimulus is the preëminent one, although it does not necessarily receive the most attention. Besides this, the discrete impression is the more sharply delineated and the exact instant of its entrance is noted with less expenditure to the subject than the momentary position of the index-hand corresponding to it. Hence the discrete impression becomes the determinate term into relation with which the indeterminate impression, namely, the corresponding scale-division must be brought.

The problem then reduces to a question of the just imperceptible time-interval. The subject is to catch the position of the index-hand simultaneous with the discrete stimulus. Adverting from one to the other with the utmost possible promptness, he will normally select a position necessarily somewhat behind, that is, somewhat later than the position to which the sound (or flash) actually corresponds. For, a certain time, however brief, being lost in the transition necessarily, in this time, minute though it be, the index-hand will have traveled over a greater or less interval, and consequently the judgment will fasten upon a position later than the true one. However, Angell and Pierce are wrong in asserting that the subject does not experience the simultaneity of the two impressions, but that he is satisfied with his judgment when he has succeeded in approximating a position of simultaneity as ascertained by the feeling of a very brief interval between the auditory and the visual impression. The subject, being acquainted with the object of the experiment, necessarily understands that the whole problem turns upon his appreciation of simultaneity, and that he has satisfied the essential conditions of the experiment only when he has succeeded in determining the position on the scale at which the index-hand seems to him to arrive simultaneously with the occurrence of the discrete stimulus. How else could the subject assume the occurrence of the two impressions to be simultaneous than by the subjective consciousness of simultaneity? What criterion has he for determining simultaneity other than the appearance of simultaneity? Now, in a determination made in conformity with the procedure of the temporal type of observer, the place at which the index-hand appears to be simultaneous with the discrete impression will be found to be, as a rule, *after*, *i. e.*, farther along the scale (in the sense of the direction of the rotation of the index-hand) than the real position of simultaneity, and for the reason that the discrete impression is at the final judgment the primary one, and a certain time-interval between it and the arrival of the index-hand at a certain point on the dial will be unperceived; hence the arrival at this point will be apprehended as simultaneous with the discrete impression.

The procedure just described is normal in determinations in which the prevailing tendency is to the temporal form of judgment; but it is of course probable that the imperceptible time-interval will occasionally be reduced to zero, in which case there will be no displacement; or that the discrete stimulus will sometimes be taken as secondary and the arrival of the index-hand at the point on the dial as primary, in which case a negative error will result. We have here then, an explanation of the occasional so-called correct judgments, though in truth a determination which actually corresponds to the real position of simultaneity is no more correct subjectively than a determination which gives the maximum displacement; for in each case there is subjective consciousness of simultaneity.

Wundt is therefore unwarranted in alleging the presence of these occasional random correct judgments as proof of the ability to experience two impressions as simultaneous, for as an experience of simultaneity it is no more trustworthy than the experience of simultaneity in those cases in which the judgment actually shows a considerable displacement.

As just said, occasional negative displacements are accounted for by the subject's inversion of the usual time-order. But what is the true account of the general prevalence of negative errors, such as shown by all the experimenters up to the present time, with the single exception of Angell and Pierce, whose results showed a prevailing tendency to positive displacements? In my account of the opposed direction of displacements I am led from the results of observation and introspection to rely upon the factor of the opposed method of observation characteristic of the spatial type of observer and I may immediately summon to support my argument the fact just cited, that Angell and Pierce, who throughout their experiments adopted the method of determination recognized by Geiger as belonging to the 'reflecting' type (already identified with the temporal type of my classification), and who were the only investigators to observe exclusively according to this method, were also the only observers who obtained throughout a preponderance of positive displacements; agreeing with the quality of the displacements Geiger found to obtain with those of his subjects who observed according to the corresponding method.

What then was the method adopted by the other investigators? Geiger, who made inquiry of two of those preceding him and even conducted a few experiments upon them in the hope of finding them of the 'naive' (or spatial) type and so confirming his observation that the 'naïve' observers show negative results, was disappointed, for at least one of the observers (a subject of Pflaum's) proved of the 'reflecting' type. Geiger therefore was led to attribute the discrepancy to the apparatus employed and concluded that the pendulum apparatus rendered difficult the observation according to the strict 'reflecting' method, owing to the difficulty of steadily fixating a single scale-division, due to the variability in the movement of the index-hand and the resulting obscuration of the scale-divisions in its course.

If the increasing strain of attention described by Wundt is responsible for the occurrence of negative displacements, it would seem to be a more potent factor in determinations of the temporal type; in which the increasing strain of attention may occasion an anticipation of the alignment. But this anticipation, as we have said before, accounts only for occasional negative displacements. But what of the negative displacements occurring in the large majority of cases? To explain these errors which are due, as we have stated, to the spatial form of judgment, it is necessary to invoke the fact insisted upon by Wundt and Geiger, that the interval in the appearance of the two stimuli is a relative, and not, as von Tschisch thought, an absolute one. It is a question of the relative time of apperception, as Wundt terms it, of the auditory and visual stimuli. Now those subjects who identify the discrete stimulus with a position of the index-hand spatially separated from the position on the scale at which index-hand and momentary stimulus are simultaneous, the attention being fixed more upon the discrete stimulus, are led by the sense of its spatial relation to the rotation of the index-hand, that is, by the feeling that the discrete stimulus somehow enters from a position somewhere in the direction from which the index-hand approaches, to identify the discrete stimulus with a position earlier than the actual one in the course of the index-hand. That is to say; to the observer of the spatial type, the traction upon the attention exerted by the feeling of the direction from which the spatially identified discrete stimulus appears to enter—the direction of entrance of the index-hand with which it is allied in space—causes the subject to displace it in the direction of its apparent entrance, thus uniting it with an earlier position of the index-hand than the correct one and giving as a result a negative error. This is the element in determining the judgment which I called factor Number 2, and which has been up to the present undisposed of.

Obviously, the greater the number of repetitions of the stimuli, *i. e.*, the greater the number of preliminary rotations of the index-hand, the greater the effect of the traction upon the attention by the discrete stimulus due to the influence of the sense of its direction of entrance; the discrete stimulus, as just explained, being identified in the mind of the subject with the direction of movement of the index-hand and having its spatial significance augmented through this association with this movement in space. In this way may be explained the experiments of Geiger in which, observing the error obtaining with each rotation from first to last, the position of apparent simultaneity was found to recede progressively, *i. e.*, to shift with each rotation to a position farther removed in the direction from which the index-hand approaches. This assumption likewise affords an explanation of the tendency to greater negative errors with the slower speeds, as shown by the experiments of the Leipsic investigators; for naturally, the slower the rotation of the index-hand, the more potent upon the attention is the influence of the traction of the discrete stimulus in the direction opposed to that of the rotation of the index-hand, and consequently the wider the displacement in the negative direction.

With subjects requiring a large number of preliminary rotations, the number of such preliminary rotations decreases with practice, hence there is here another factor explaining the greater positivity upon practice, as found by Angell and Pierce and by Geiger. For, the greater the number of revolutions of the index-hand, the more effectual is the influence, as just said, of the sense of traction in the direction from which the complicating stimulus seems to enter; that is, the greater the negativity. With practice the amount of the negative error decreases as the number of rotations necessary to a determination decreases, independently of the change of the subject towards the temporal type. So upon my hypothesis of a space judgment to account for the presence of the negative error is afforded the solution of the problem of the influence of practice in decreasing the negativity of the results.

I have explained Angell and Pierce's positive results as due to their usage of the temporal type of observation. But other influences tending to bring about positive errors in their results are these: first, they did not take account of the lag in the complicating stimulus due to the interval of time required for the hammer to act and for the transmission of the sound waves from it to the subject's ear; and secondly, the conditions of their experiments were especially favorable to the forward, over-reaching movements of the eyes, caused by the impetus received from the downward course of the index-hand as it enters upon the arc constituting the scale; the latter, as explained before, being placed below the center of revolution and in this regard being unlike the position of the scale with most of the other experimenters. The fact that Angell and Pierce noted no influence of speed is also due to their method of experiment, for speed is of little or no moment in observations of the temporal type, since here it is a question only of the time between a given stimulus and alignment, and the distance traversed by the index-hand in that time is of little consequence. In the spatial judgment, on the other hand, this distance is noted and becomes influential.

The effects of the temporal and spatial methods on the relative distribution of attention to the discrete impression and to the alignment, are yet to be considered. In the temporal form of judgment the attention is maximally on the alignment of the index-hand with the selected point on the dial, for the noting of that is the task to be executed immediately upon the occurrence of the discrete impression. In the spatial or 'naïve' form of judgment the attention is more evenly distributed between the two factors, but is greater if anything to the discrete stimulus, since it is that which picks out a position of the index-hand. Hence it would be expected that voluntary direction of the attention in a way characteristic of the one or the other of these forms of judgment would incline the judgment towards that form, as has been actually found to be the case by Stevens, Titchener and others.

An important point requiring consideration is the hitherto unrecognized question of the possible influence in the experiments earlier conducted of a factor analogous to my 'A' and 'B' directions of setting out and to Klemm's directions of approach from the two sides. We have seen the very striking effect of the position of setting out upon the resulting judgment, as shown in my experiments (see Tables 3 and 4). Now, the influences of the positions of setting out are here equally balanced on the two sides of the goal and cancel each other in the averages. But what of the influence of the position of setting out upon the results of Wundt, von Tschisch, Pflaum, Angell and Pierce, and Geiger? For, if such an influence were present and not taken into account, operating at one time to displace the judged position in one direction, at another perhaps in the other direction, the consequence of the neglect of so considerable a factor is to render uncertain the significance of the nominal results. Obviously there was such a factor present, for since a determination was not made until after several rotations, particularly in the case of the 'naïve' (spatial) observer, it is clear that corrections were made and there was therefore a point of setting out. Now it is not to be supposed that the subject had always a correction of equal magnitude to make, that he had always to advance or withdraw the position of his determination (depending on whether it appeared to him in front of or behind the new position) by the same amount with every final judgment. Nor is it to be assumed that the position of his tentative first judgment was always upon the same side of the final position in every determination. Yet in the case of some particular subject and rate one or both of these conditions might be realized. Clearly then there was in the earlier investigations the factor corresponding to my 'A' and 'B' factor and experiments conducted without regard to it fail to take account of one of the most potent of the controllable factors occurring in the complication experiment.

To summarize briefly the chief points which my investigation brings out: it shows first the fact that contrary to the implication of Wundt, a displacement occurs where the discrete and the serial stimuli belong to the same sense realm, and even to contiguous portions of the same sense organ, as proved by those experiments in which I employed a momentary light-stimulus instead of the disparate impression employed by preceding experimenters. In the second place, it brings out the difference in the determination depending upon the side ('A' or 'B'), of the scale from which the subject set out, and the distance of the position of setting out from the terminal position.

And finally, I have shown how the two types of observation found by Geiger, the effects of attention to the discrete impression or to the serial impression, the occurrence of positive displacements predominantly with some subjects and of negative with others, and the positiving effects of 'practice,' are all brought into agreement and made intelligible by the discovery and analysis of the distinction between the 'spatial' and the 'temporal' forms of judgment.

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